UNITED STATES DISTRICT COURT
FOR THE DISTRICT OF COLUMBIA

MORPHOSYS AG, a German Corporation
Plaintiff,
vs.
CAMBRIDGE ANTIBODY TECHNOLOGY LIMITED, an English company
Defendant.

Case No. 1:00CV00146(JR)

HIGHLY CONFIDENTIAL - SUBJECT TO PROTECTIVE ORDER

VIDEOTAPED DEPOSITION UPON ORAL EXAMINATION

OF

DR GREGORY PAUL WINTER Continued

On Monday, April 29, 2002
Commencing at 9.10 am

Taken at:
Crowne Plaza Hotel
Downing Street
Cambridge
Cambridgeshire CB2 3DT, England

Reported by: Thelma Harries, MBIVR, ACR

A P P E A R A N C E S:

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BY : JOHN S SKILTON, ESQ

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BY : TIMOTHY J VEZEAU, ESQ
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A P P E A R A N C E S: Continued

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I N D E X

DEPONENT:

DR GREGORY PAUL WINTER

EXHIBIT INDEX

PREVIOUSLY MARKED

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9.10 am
VIDEOGRAPHER: Here begins videotape number 1 in the deposition of Dr Greg Winter in the matter of MorphoSys AG, a German corporation versus Cambridge Antibody Technology Limited, an English company, in the United States District Court in the District of Columbia, case number 1:00CV00146(JR).
Today's date is 29th April 2002. The time on the video monitor is 8.10. I apologise; I obviously haven't changed the time since British summer time. I apologise for that.
The video operator today is Jools Viner and this deposition is taking place at the Crowne Plaza Hotel, Cambridge, England.
Counsel, please voice identify yourselves and state whom you represent.
MR SKILTON: Yes. For the plaintiff, MorphoSys, John Skilton. And with me today is Colin Sandercock, who is not in the room but soon will be, Michelle Umberger and Dr Bernhard Virnekas.
MR VEZEAU: On behalf of the witness, Dr Winter, and Cambridge Antibody Technology, my name is Tim Vezeau. With me are my colleagues Jane Choi and Dr Clough of Katten Muchin

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Zavis Rosenman, and Sean Walton of Mewburn Ellis.
VIDEOGRAPHER: Thank you. The court reporter today is Thelma Harries. Would the reporter please swear in the witness?
(Witness sworn)
MR SKILTON: Good morning, Dr Winter.
Before we begin, let me make a record of what it is we've received by way of documents in the last week, and hope that it will conform with counsel's records.
On Friday we received documents bearing the Bates stamp number of CM040126 through CM040554.
By fax on Friday afternoon we received documents bearing the Bates stamp number 9000908368 through 8388 and 9000908392 through 8423.
Are those the documents that we have been served with up and to the point of this deposition?
MR VEZEAU: I assume you have been served with them. And beyond that I can't say.
MR SKILTON: And then this morning we received an additional set of documents of

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CM040555 through CM040623.
Those are the documents we've received, and my question is: Are there other documents which will be produced in the course of this week that have not yet been produced, Mr Vezeau?
MR VEZEAU: As far as I know, you've received all the documents that will be produced. Of course, something may come up and we'll deal with it then but, so far as I know, you have what you're going to get.
MR SKILTON: Although Mr Vezeau noted his presence, I note the presence in the room of attorney Sean Walton, who will be a witness in the course of these proceedings in England, and the record should note my objection to his presence.
MR VEZEAU: And the record should also note that, during the course of various depositions in this case, certainly witnesses have sat in on depositions who will be witnesses in this proceeding, including Dr Virnekas who happens to be here today.

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DR GREGORY PAUL WINTER
having been duly sworn
was examined and did testify
as follows:
EXAMINATION
BY MR SKILTON:
Q Would you state your full name for the record, please?
A Gregory Paul Winter.
Q Dr Winter, I'm going to attempt to ask clear questions. You are on a video record and if and to the extent you can take your time in answering the questions it will assist both the court reporter and the video record.
A So what do you mean by that? You'd like me to take my time in answering questions?
Q This may simply be a speech issue but, on occasion, the transcript is difficult to follow or the tape is. It may be simply a matter of speed of speech.
A Oh, I see. I'll try to take my time.
Q Thank you very much.
Dr Winter, when was the first time you met Dr George Smith?

A I don't believe I've ever met George Smith.
Q When was the first time you became aware of the work of George Smith?
A I think it was in about 1985 or 1986.
Q How did you become aware of his work?
A It was brought to my attention by a post-doctoral worker in my laboratory called Dr Phil Jennings.
Q Was there a particular reason why Dr Jennings brought this work to your attention at that time?
A There probably was, although I'm struggling to remember exactly why. He had been interested in the evolution of influenza virus haemagglutinin, and he was facing expressing the influenza virus haemagglutinin in some kind of expression system. And I think at that stage we had been thinking about expressing the haemagglutinin in eukaryotic cells, which would therefore give you properly glycosylated haemagglutinin.
But he came across this paper by

George Smith and was very excited by it and suggested that we should try to express the influenza virus haemagglutinin in the way that Smith had done.
Q In particular, what do you mean by that in the way that Smith had done?
A To put it on the -- to try to display the haemagglutinin on the surface of bacterio phage.
Q Had you personally ever supervised a project before that time using bacterio phage?
A I had supervised a project, and I had also had extensive experience with bacterio phage myself, but for DNA sequencing. So we were not involved in expressing proteins on the surface of the bacterio phage; we were much more concerned with growing the bacterio phage as a source of single stranded DNA.
Q Did you take the time at that time to read the Smith article?
A I did read it.
Q Did you, by any chance, keep a copy of the article that you read?
A No.
Q What did you conclude as a result of

your reading of that article at that time?
A For the particular purpose that was suggested, I didn't think it was a good idea. In fact, I thought it was unlikely to work.
Q What was the purpose that was suggested that you did not think was likely to work?
A Well, that article describes the introduction of a peptide into part of the gene III protein of the filamentous bacterio phage. There was no evidence in the paper that that peptide was folded up. In fact, it seemed to me very unlikely that it would be.
The paper related to the use of -- was using polyclonal anti-serum to detect the expression of the Smith peptide, and I was very doubtful that you could effectively insert one protein into the body of another protein and to have both proteins fold up.
Q Did you thereafter do any experimental work to determine whether or not this could be effected?
A No, I didn't.
Q When was the first time --
A I mean, at that time we didn't do

any. Obviously later on we did.
Q In fact, that was where I was going.
A Okay, yes.
Q When was the first time that any person, yourself included, in your lab made an effort to express or display on bacterio phage antibodies or antibody fragments?
A The first time we made an effort to display on bacterio phage was the work of John McCafferty.
Q Was this work, that you're now describing, work that was done under your supervision?
A The work was done under my supervision, but we have to be careful what that means.
It was my laboratory. John McCafferty was working in my laboratory, but it was a collaboration with David Chiswell of Cambridge Antibody Technology, a company we'd set up.
So this was -- this was a situation in which we were undertaking this work jointly, but I was responsible while he was in my laboratory for what he was doing.
Q Did you yourself do any of the

experimental work with respect to this project?
A I did not. Some years ago I was attacked by a lunatic and I lost the use of my right arm for experimental purposes.
Q Did you directly supervise any of the experiments done in reference to this project?
A Yes, I did.
Q When was it in point of time that John McCafferty first started his work in your laboratory?
A I believe he started the work in early January. I can't remember whether it was -- it was basically after the Christmas holidays and when the lab came back, and I can't remember exactly when that was, but it was very early January.
Q Of what year?
A This must be 1990.
Q In preparing for today's deposition did you examine any documents?
A Yes.
Q What documents did you look at, Dr Winter?
A I looked at the patent. There was the US specification. I also looked at the

testimony of Tony Pope, Ron Jackson and Kevin Johnson, and I think I probably also looked at the testimony of Andrew Griffiths.
Q How about John McCafferty?
A Ah yes, I looked at the testimony of John McCafferty.
Q Did you look at any contemporaneous -- by that I mean documents made during the period of the work of McCafferty in your lab concerning phage display of antibodies?
A I wasn't given any of those to look at, and I didn't. I don't have any.
Q Did you look at Dr McCafferty's notebook concerning this work?
A Sorry, you mean recently?
Q Yes.
A No.
Q It is my recollection from the last deposition that, in the course of your work in various cases, you prepared a chronology. Is my recollection correct in that regard?
A You'll have to explain this --
MR VEZEAU: I'm going to --
THE WITNESS: Sorry.
MR VEZEAU: I'm going to object to

that question as lacking a proper foundation.
MR SKILTON: I'll rephrase it.
BY MR SKILTON:
Q At any time have you prepared a chronology of events relating to matters concerning phage display of antibody work in your laboratory?
A A chronology relating to the work within my laboratory? Or the work of the field as a whole?
Q Well, either or both?
A I don't recollect a chronology, but it's not impossible that I have written something at some point. But I think it's much more likely to be a chronology relating to, for example, in the preparation of a review for the field, more to do with the relationship of the repertoire technology and Richard Lerner's group.
Q Was this the technology that was involved in the interference, the Scripps interference proceedings?
A Yes.
Q Did you prepare a document, to your recollection?
A I think I prepared a document.

I prepared a document. Whether -- and it probably would be called a chronology, but it does not relate to the display on phage, to the best of my recollection. I believe this pre-dated the work on phage and was related to the work on antibody repertoires.
I don't specifically recollect writing a chronology on phage display.
Q Have you made any effort, prior to today, to reconstruct that chronology as it relates to work in your laboratory?
MR VEZEAU: What chronology?
MR SKILTON: The chronology relating to the work of phage display of antibodies in the Winter laboratory.
THE WITNESS: Have I made an effort to reconstruct a chronology? Not really.
I have remembered things. I have thought back over that period, but I have not done it in the form of a chronology.
I've just thought about the events that happened and the reasons we did things, as, obviously, reading the testimony of the others, John McCafferty, and my own recollections, it, sort of, stimulates your brain cells and you start

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thinking about, you know, what happened and when. But to refer to it as a chronology I don't think would be correct.
BY MR SKILTON:
Q How much time have you spent in preparing for today's deposition as you have earlier described?
A I had a meeting with the lawyers several months' ago, and then I had a meeting -- that meeting was about -- that meeting was several hours. And then I had a meeting last week with lawyers, and that lasted several hours again.
Q And those lawyers include Mr Vezeau?
A Yes.
Q Ms Choi?
A Yes.
Q Mr Clough?
A I'm sorry?
Q Mr Clough?
MR VEZEAU: Dr Clough.
THE WITNESS: Oh Clough. Right, sorry, he's called Clough, is he? Okay, right. Sorry, I thought he was -- we would call him "Cluff", if you spell his name like that.
BY MR SKILTON:

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Q And Attorney Walton?
A I'm sorry?
Q Attorney Walton?
A Oh yes, yes. The person you objected to, yes.
Q Any other persons?
A There was somebody else but I've forgotten his name.
Q A lawyer?
A I think he was.
Q Had you met him before?
A No.
Q At some point in time your lab then set about the job of determining whether or not to display antibodies on bacterio phage, is that correct?
A Sorry, could you repeat that question?
Q Yes. When was it in point of time that your laboratory first became interested in expressing antibodies or antibody fragments on bacterio phage?
A That sounds like a different question.
Q It probably was. You asked me to

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state a question and I did. I'll withdraw the question.
A You re-stated the question. Okay. So on that re-stated question: When did we first become interested?
I think we were musing with the idea when Andrew Griffiths was about to join the laboratory. So we're talking about 1988.
Q What caused you to muse about the idea?
A Well, when someone new comes to a laboratory we obviously have to think about the kind of project we're going to do, and what I try to do is to think about interesting projects that also have an opportunity of success, and we had a discussion about the use of -- about the use of bacterio phage at that stage. I don't remember very much about it because it's a long time ago.
The reason that we started thinking about it was because we had already started the work on making antibody repertoires, and we wondered whether it might indeed be possible to express antibody fragments on the surface of phage.
Q Did you give Dr Griffiths an

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assignment at that time in 1988?
A He did have an assignment. It was to humanise an antibody.
Q Did you ask him to start a project with reference to the expression of antibodies on bacterio phage at that time?
A No.
Q Are you aware of any work of Dr Smith that you reviewed in 1988 after that first review of the 1985 article?
MR VEZEAU: Would you repeat that question, please, Thelma?
COURT REPORTER: Certainly.
"Q. Are you aware of any work of Dr Smith that you reviewed in 1988 after that first review of the 1985 article?"
THE WITNESS: I don't specifically remember reviewing any work of Dr Smith until the time of -- until about late '89, but somehow I was aware, and I don't know why I was aware of it, of the possibility of expressing antibodies at the end terminus, or the possibility of expressing something at the end terminus of filamentous bacterio phage rather than in the middle of the gene, and that actually gave me an extra level of

confidence.
Now, I don't know why I was aware of that. I assume that I either read or my attention was drawn to the paper of Parmley and Smith.
BY MR SKILTON:
Q A 1988 paper?
A A 1988 paper, but I can't say when I read it. I can't even specifically remember reading it at that time.
Q You have read it since?
A I have read it since.
Q In fact, when was the last time you read that Parmley and Smith paper?
A The whole paper in detail?
Q Yes.
A Or you mea just bits of it?
Q Well, let's talk about the whole and then we'll go to bits. First the whole.
A I think I read the whole paper at the time that we filed our patent, in about probably '90 or '91, and I read it carefully then.
And, more recently, I read that paper probably a few months' ago but, essentially, just looking at one or two bits of it.
Q Had you read it prior to the time of

the filing of your patent, if you recall, either in part or whole?
A Which filing?
Q Well, you mentioned the nineteen --
A Well I'm talking about the final filing. I know I read it before the final filing, so that would be '91, I guess. So it would have been read before the final filing. I know I read it carefully before then.
I don't remember reading it carefully before that period, but I was aware of it. I was aware of the possibility of expressing peptides at the end terminus of gene III before that time.
Q Do you attribute that particular aspect of your work to Dr Smith?
MR VEZEAU: Excuse me, I think that lacks a proper foundation. "The aspect of your work", I don't know what you're referring to.
BY MR SKILTON:
Q Do you under the question, Dr Winter?
A Not really. I'd like you to repeat it or in a different form.
Q Yes, I'll try it another way.

Did you learn anything from that paper that you used in your work in your laboratory?
A Did I use anything from that paper? Well, to the extent that I was aware of placing peptides at the end terminus of gene III, before McCafferty came to the laboratory, and given that McCafferty placed the antibody fragments at the end of gene III, then I assume that, yes, that aspect of Smith's work was used.
MR SKILTON: I'm going to place in front of you a document, which we'll ask to be marked as exhibit 1 in this deposition.
(Exhibit 1 marked for identification)
BY MR SKILTON:
Q Dr Winter, this is a document which bears the Bates stamp numbers of CM040353 through CM040358. It's a lengthy document. My first question is: Do you know who wrote it?
A I'd have to read it.
Q Can you read the first paragraph, if that helps? I won't be asking you the substance of the document, unless it's a document that you are personally familiar with.
MR VEZEAU: Dr Winter, if you need

to read any document during your deposition in order to answer a question, you feel comfortable in doing so.
THE WITNESS: (Witness reviewed the document) I'm going to have to read this document before I comment on it. Do you want me to do it now?
BY MR SKILTON:
Q I won't take the time at this deposition to do it, other than to ask you one simple question. Have you ever read this document before?
A Until I read it, I don't know.
MR SKILTON: Let me put a series of documents in front of you. I don't wish to have them read. I want to have them marked for identification purposes and, if and to the extent I ask you any substantive questions, I'll permit you the opportunity to read it. So let's mark these documents in sequence.
(After a pause) Let's go off the record and conform the exhibits.
VIDEOGRAPHER: We're going off the record. The time is 8(sic).39.
(A short recess at 9.39 am)

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(Resumed at 9.43 am)
(Exhibits 2, 3 and 4 marked for identification)
VIDEOGRAPHER: We're back on the record. The time on the video monitor is 8.43.
BY MR SKILTON:
Q Dr Winter, I have placed in front of you additional documents, which were contained and taken from a production described earlier on this record, and I will specifically now identify for the record exhibit 2 which, on its face, states drafts 2 (GW15/8/90) and bears the Bates stamp number of CM040359 through 362.
Focusing on that document, is this is a document you recognise?
A I mean, all I can do at the moment is to look at the title of it, the fact that there are several names on it. It looks as though it is a draft of the McCafferty paper, but I would have to read it carefully to be sure of that.
Q Do the initials on the top represent your initials, to your understanding?
A GW, yes. You mean GW 15/8/90?
Q Yes.

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A Yes.
Q Does that help you in any way to identify who prepared this document?
A It would suggest I prepared it.
But I should also point out that, if you had simply taken those initials off one document and stuck them on top of another document, I wouldn't actually know for sure exactly whether that correctly represented the draft of the paper; all I could say is it appears to be a draft of the paper but I can't guarantee that until I read it and, even then, I may not be able to. It's a very long time ago.
Q Do you have any knowledge as to where these particular documents -- now I'm referring to 1 and 2; exhibits 1 and 2 -- that you have quickly looked at and have not read, where they came from?
A I don't know.
Q Do you know whether or not these documents were sent to CAT's lawyers, at any time?
A I have no idea.
Q Let's finish the identification of these documents then for the record and we'll move on.

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Exhibit 3 is in front of you. It purports to be draft 3 (DJC 15/8/90) and bears the Bates stamp numbers CM040363 through CM040368. Do you recognise the initials DJC?
A Yes, I do.
Q Whose initials are these?
A David Chiswell.
Q Without comparing it, does this also appear to be a draft of what ultimately became the McCafferty paper?
A It does, but there's actually...
What surprises me about this is that the dates are both the same; there's GW 15/8/90 and DJC 15/8/90. I haven't compared the two, and I know we were working hard to get this paper written, but I'm surprised that, if there significant changes between them, that this was done in a day.
Q You said you know "we were working hard to get the paper written"?
A Yes.
Q What did you mean by that comment?
A That we were working hard in the sense that we were -- that we started from a draft. I can't remember who wrote the first draft.

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I suspect it was not me, but I'd have to read the document carefully.
We had a lot of discussion about what should go in the paper, the journal it should go to, and we were in the process of describing the work and crafting it to the journal where we wanted to send it.
Each journal has a different style. I think in the end we decided that we would have a go for Nature, and so we had to put it in that style. And, in fact, it's quite difficult to get all the information into a Nature letter, which was what was intended, and so I think we had to work quite hard on trimming it at the very beginning and deciding what we were going to leave out.
Q Was there any time urgency concerning the submission of this paper?
A I think, as a scientist, there is -- if you've got an exciting result, then you want to get it out in case someone accidentally -- you know, perhaps someone else is working on something similar. You don't know. So, for that reason, we decided to move.
We also wanted to get publicity for the company, Cambridge Antibody, which we'd just

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set up. It was a fledgling company and we thought this would be a great paper for that purpose.

Q Was there any consideration given by you personally, in terms of the timing of this publication, to get it published prior to any specific laboratory publishing its work?

A The main consideration we had was that we were in scientific competition -- well, it's the main problem I had, was I was in scientific competition with Richard Lerner's lab, and I had no idea whether he might be doing something similar. That was a consideration.

A second consideration was that we thought we had a reasonable prospect of getting it out that year; in other words, within the year 1990. If we delayed much longer we could find it coming out in '91, and, in terms of establishing a scientific priority, it was therefore very important to get this paper out in 1990.

So I think there were those considerations. And I think the immediate pressure on us at that point was more to do with getting it out within the time year 1990.

Q What do you mean by "scientific priority", Dr Winter?

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A Well, we thought that, having established that you could display antibodies on phage and select them, we believed that this would be the solution to selecting binding specificities from repertoires of immunised and perhaps even unimmunised V-genes, and so we wanted to show that we had done this first.

Q Was the issue of patent protection a consideration with reference to publishing this paper at this time?

A I don't think there was a specific issue of patent protection because, if anything, disclosing work makes it more difficult patent-wise.

Q Do you recall when it was in point of time that you first disclosed your work to your patent lawyer?

A I don't remember.

What happened was that David Chiswell and -- well, particularly David Chiswell was dealing with the patent lawyer. He, I understood, had talked to the patent lawyer certainly before this date in August, and we had filed, to the best of my recollection, at least one priority document before this date. But I didn't,

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in fact, have -- I don't think at that point I had any discussions with the patent attorneys.

The priority, really, was that David Chiswell was concerned with the commercial issues of drafting a patent and I was concerned with the science.

Q Did you have any responsibility with respect to that first disclosure to the patent lawyer that you were aware had been made?

A I'm sorry?

Q Did you do anything with reference to that disclosure? Write a document? Be interviewed?

A Sorry, which disclosure?

Q The disclosure to the patent lawyer that preceded this draft?

A It wasn't disclosed. I mean, it wasn't disclosed. It was secret.

Q Yes, but I'm now talking about the inventive disclosure.

A Did I have discussions with the patent lawyer?

Q Yes.

A I don't think I did.

Q Did you ever have discussions with

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a patent lawyer with reference to prosecution of the application that ultimately in the United States became the 108 patent?

A The 108 patent, as far as I recollect, was filed a year later, in July 1991. Is that the one you're referring to?

Q I believe that the filing you're now referring to is in the European patent office, but let's use that date, if we can.

A Sorry, the July nineteen?

Q July 1991.

A July 1991. Yes, I was involved by that stage very much in formulating -- well, having discussions with patent lawyers and helping, along with David Chiswell, to formulate the claims, also in conjunction with the patent lawyers.

Q Who were your patent lawyers at that time, July of '91 and prior?

A The patent lawyers were Mewburn Ellis.

Q Was there a person in particular that you dealt with at Mewburn Ellis?

A The person I think we mainly dealt with, or certainly the most intensive discussions I recollect, was a partner of the firm called Ian

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Armitage.
Q Would you spell that name, please?
A Armitage, A-r-m-i-t-a-g-e.
Q First name?
A Ian.
Q During this period were you dealing with a lawyer by the name of Sean Walton?
A We might have been, but actually I don't remember him then, but he could well have been around. As I say, the main personal we were dealing with was Ian Armitage. I came to know Sean Walton later.
Q Can you be more specific?
A Not really, no.
Q Did you, at that time, draft any documents for Mr Armitage? That time now is July of '91 and prior.
A July '91 is the period I particular remember. That isn't to say that I might not have been involved in drafting documents before that, but I simply don't remember it now.
But I do recollect in July '91 having extensive meetings at Mewburn Ellis's offices and working on the draft on a computer screen, but I don't remember at that stage having

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a copy of that draft myself.
It was a very large document. There was a lot of bits of -- there were a lot of examples going into it. In fact, my main responsibility, again, was on the science to make sure that we were pushing ahead at the work, because we were in the process of making antibody repertoires and selecting them and we wanted to try and get all these examples in the patent. So I was largely responsible, I think along with Ron Jackson, for getting the work written up in the form of an example that we could put into the patent.
Q Jackson worked on it with you?
A I believe so.
Q Who else from either the MRC or Cambridge Antibody worked with Mewburn Ellis at this time?
A It's possible John McCafferty may also have been there, but I particularly remember David Chiswell, myself, and I'm fairly sure Ron. I think John may have come to some of the meetings but I don't think he came to all of them.
Q Let's use that date of July of 1991 and see how specific we can be.

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I take it around that date, and prior to the date of the actual filing, there were meetings involving the drafting of the patent application?
A There may well have been meetings. I don't remember any of those meetings with Cambridge Antibody.
I do remember going to people within my own laboratory and talking to them and trying to make sure that we got the data down for taking it across to the patent attorneys.
Q You earlier stated that one of the things you were particularly concerned with were the examples in the patent, is that correct?
A I was concerned with some of the examples. There were others which, in fact, I didn't have much to do with, which were work being undertaken in Cambridge Antibody Technology.
Q Are you able here to reconstruct what examples you personally worked with in reference to this application?
A I'd have to see a copy of the filing and actually go through it carefully, but I can remember some of them.
Q Would you, please?

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A But I can't remember... Well, obviously there was the first example; in other words, the display of the anti-lysozyme, the VH and ScFv on the surface of phage.
Q This was the McCafferty work?
A The McCafferty work, correct.
Q And this was the initial work?
A That was the initial work.
Q And this was what had led to the filing of the first disclosure?
A Correct.
And there was the other example that I took responsibility for, was the Clackson work on selecting anti-phenyloxazolone phage antibodies from an immunised animal.
And the third one I remember having involvement with was the work on naive libraries of antibodies, which had been work by Jim Marks and Hennie Hoogenboom and Andrew Griffiths.
But there may have been a number of other things in there. I'd really have to go through each example and describe my involvement.
Q I'm going to, if I may, complete the record on identification of the documents that have been in front of you and not yet identified.

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Exhibit 4, do you have that in front of you?
A Mmmm.
Q That appears to be, on its face, a document styled "phage antibodies", etcetera, with authors, including yourself, listed, and bears the Bates stamp number of CM040369 through CM040378. This appears to be a draft of the paper, does it not, Dr Winter?
A It does appear to be.
(Exhibit 5 marked for identification)
Q In front of you is exhibit 5, which is a document that bears the Bates stamp number CM040379 through CM040385. Again, this appears to be a draft of the paper, does it not?
A Yes.
(Exhibit 6 marked for identification)
Q And then I place in front of you a document which is separately marked -- whether it should be or not I think the record will ultimately have to be clarified -- is exhibit 6, which bears the Bates stamp numbers CM040386 through CM040392.
Do you recognise this set, if you will, of documents just by looking at them quickly?
A It looks like a series of pages

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taken from the work at random.
Q All right. Let me go back, if I may, to your participation in the drafting of examples for the patent.
Did you keep a file with reference to the work that you did in assisting your patent lawyer?
A No.
Q Is there any way, that you're aware of today, to reconstruct your contribution with reference to any of the drafting of this patent?
A I think the only way to do it would be for me to look at the patent and just tell you, to the extent that I can remember.
Q At any time did you make an effort to identify separately who invented what with reference to this patent?
A We had a discussion with our lawyers at that stage as to who the inventors were. And the way this worked was that quite a few people in the laboratory had been involved in the work and it wasn't -- because I'm not a lawyer, we had to take advice on, of those people, who were inventors and who were not.
So that what we did was we -- the

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way we decided to do this was to write the patent with the examples that we felt were necessary to support the patent, and then we looked at the role of -- we took a look at each claim in the patent and asked who were the people who'd been involved in each of those claims. And that was a process which involved myself, David Chiswell and the patent lawyer.
Q The patent, of course, is written with a number of people listed?
A Yes.
Q And the patent does not separate out the individual contributions of any of those persons?
A That's true, but that's fairly normal in the case of a patent.
Q My question is: Is there any way, that you're aware of today, to reconstruct the individual contributions of each of the named inventors?
A I think the process would have to involve, essentially, myself and David Chiswell going through the document.
Q Have you ever been asked to do that?
A No. But I have to say that, of

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course, that, along with other evidence, even files, is just an attempt to get at an event which happened a long time ago.
Q Well, it was easier to do it a long time ago than it is today, I assume. Would you agree with that?
A Well, I don't know. I mean, I think I'd just have to look at it. I mean, I think that the decisions we certainly felt then were quite defensible.
Q Did you, if you recall, ever -- and by now I've, sort of, asked this in stages. At the time of July of 1991, did you prepare a document which separated out the individual contributions of each of the named inventors?
A I do not remember preparing a document of that sort.
Q Have you ever seen a document which did that?
A I don't think so.
Q Have you ever participated in the preparation of any presentation as it relates to who invented what in what became the 108 patent?
A I have no recollection of a presentation.

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Q And --
A I should add, though, that one of the things that I had to do was to explain to various people in the laboratory why some people were inventors and why some people were not. So, although I didn't write a document on it, it was pretty clear in my mind and I was able to explain to them.
Q If you were asked to do so today by looking at the patent, would you be able to do so?
A I believe I could get -- I could identify several of them. I'm not sure I can, at the moment, say that I could do all of them, but there's a chance I might be able to.
But I have to point out that the process was a joint process, so I can just remember my own involvement in it, to some extent, and it would be helpful to talk to Ian Armitage and David Chiswell in order to reconstruct, you know, what hopefully would be the truth.
Q So the process you would envision to answer that question would be for you to read the patent, to engage in a discussion with Chiswell and Armitage, and try to reconstruct who invented what?
A I think that would be the way

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I would -- that's one way you could get at it, yes.
Q And is it correct to say that, up until this very point in time, you were never asked to do anything like that by anybody on CAT's behalf?
A I have not been asked to do anything like that, no.
Q Do you know whether attorney Armitage is still with Mewburn Ellis?
A I do not, no.
Q When was the last time you talked with him?
A I think it was some time in the early nineties. It was in Japan.
Q What was the occasion, if you recall?
A There was a conference in Japan and he was in the audience.
The reason that Cambridge Antibody had chosen Mewburn Ellis was because I had previously been an advisor to a company called Scotgen.
Q Could you repeat that, please?
A Scotgen.
Q S-c-o-t?

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A S-c-o-t-g-e-n. And Scotgen was -- it was a small biotech company, that later died. Some time in the early nineties had died. But I'd been very impressed by the work done by Mewburn Ellis, in particular Ian Armitage, in discussing humanising antibodies.
Scotgen was involved in making a business out of an earlier technology I'd invented, which was a process of humanising antibodies, and Ian Armitage was the agent they'd used and was extremely impressive and, therefore, I suggested that Cambridge Antibody should use him, and they did.
Q Are you aware of whether or not Mr Armitage made any notes with reference to this issue of who invented what?
A I have no idea.
MR SKILTON: Mark this, please.
(Exhibit 7 marked for identification)
BY MR SKILTON:
Q Dr Winter, I have placed in front of you a document which purports to be a press notice of MRC and bears the Bates stamp number of CM040126 through CM040128. Do you remember ever seeing this document before?

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A I probably saw it at the time. I don't specifically remember this document. It appears to be a press release from the MRC.
Q Would you take your time here and read it and then I want to put some questions to you?
A (Witness reviewed the documents) Yes, I've read it.
Q This appears to be a document that issued on or about December 5, 1990. Do you read it that way, looking at the last page?
A Yes, it appears to be, yes.
Q And it was a written in anticipation of a paper to be published on December 6?
A I don't know the publication date of the McCafferty paper but, if it is the 6th, you know, that would...
This does relate to that paper, but exactly the precise dates of publication of this and the paper I couldn't be sure of.
Q This document refers to a challenge -- the first page, if you will -- from Dr Milstein. What, if any, relationship did this challenge from Dr Milstein have to the McCafferty project that ultimately resulted in the paper?

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A Well, it says here that the challenge, according to this press release, is, Could we devise a method of making artificial human antibodies entirely outside the body without the use of human or animal immunisation? And it represents the McCafferty paper as being a step towards that goal.

Q Is that a fair statement of, in fact, why it was in point of fact that the MRC laboratory first commenced to do the phage display of antibodies?

MR VEZEAU: Thelma, why don't you read that one back, please?

COURT REPORTER: Certainly.

"Q. Is that a fair statement of, in fact, why it was in point of fact that the MRC laboratory first commenced to do the phage display of antibodies?"

THE WITNESS: You talk as though the MRC laboratory is something distinct from my own group. What do you mean?

BY MR SKILTON:

Q Maybe I have misstated a distinction.

A Right. So that what I think you...

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The MRC laboratory, apart from myself, was not involved in undertaking the work on antibody repertoires. It was only my group.

So if I rephrase that question as: "Is this a fair summary of why we had done our work?", I have to think carefully. I mean, obviously...

Are you saying, "Is it a fair" -- are you saying that we did our work in response to Milstein's challenge? Or are you saying that we did our work as basically a step in the way of making antibodies without immunisation?

Q What, if any, relationship did Milstein's challenge have to the commencement of the work that McCafferty did on phage display?

A Milstein's challenge was not directly -- was not the motivation behind the work on McCafferty. But Mil --

Q What was -- I'm sorry.

A But Milstein -- the problem we were dealing with is that there were things out there in the public domain and Milstein had actually put in his Croonian Lecture of the Law Society towards the end of his paper the idea that one might be able to make antibodies by some synthetic process. And

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that was out there, so that he'd put that work in.

But Milstein was well aware of what I was doing at the time he was the head of the division, and I used to talk to him and keep him fully up-to-date. He was very excited by the work.

Q What was the motivation, the particular motivation for the McCafferty?

A The motivation for the McCafferty work was to find some way of dealing with the very rare phages with binding activities that emerged as a result of making combinatorial repertoires.

Q I want to refer to the bottom paragraph on CM040127, which purports to be a quote of you. Are you following me?

A Yes.

Q I'll read the reference point of the question. "'This work is promising' says Dr Winter 'but further steps are needed.'"

"This work" is referring to what, as you read this document?

A It's referring to the use of McCafferty. In other words, the display and selection of folded antibodies on the surface of phage.

Q What are the "further steps" that

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you're referring to in this quote?

A Well, it says here, "For example, it will be necessary to make diverse libraries of phage antibodies, and to find ways of mutating and then selecting for those that have improved binding activities."

Q Was your laboratory at that time working on making of diverse libraries?

A I'll just have to think about the dates here.

(After a pause) We had already described the previous year making of repertoires of antibody heavy chain genes from immunised animals and expressing those in bacteria, and we continued to undertake work in that general direction; in particular, trying to make combinatorial repertoires by -- let's say, random combinatorial repertoires by taking a diverse set of heavy chain genes and a diverse set of light chain genes and be able to stitch these together in an entirely random manner. And so we used a PCR strategy, and that was work that Tim Clackson was working on.

Now, I can't remember where that work was at the time that this press release was

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issued, but it had certainly been started.
Q Dr Winter, I'm placing in front of you what, on its face, appears to be an article which appeared in Nature and was earlier marked in these proceedings as McCafferty exhibit 11. (Same handed) Do you recognise this article?
A (Witness reviewed the document) This appears to be a copy either of the article that emerged or, conceivably, a proof copy. No, it must be the article because it's on 6th December and the publication date is given.
Q And this is the article, would you agree, that's referred to in the press release?
A Yes. It is, yes.
Q Now, your work, earlier work on libraries had also been published, had it not?
A Yes, it had.
Q And, as you sit here today, what you're saying is that you can't put a direct time line on the status of additional work with respect to libraries, is that correct?
A At this point, you know, at this particular moment in time I can't recollect exactly where our library work was at that point, but we were certainly working on it actively.

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Q And the "for example" sentence, it refers in part, does it, to the ongoing work?
A I'm sorry?
Q Of your laboratory, as it relates to libraries?
A I'm sorry, which sentence are you talking about?
Q I'm going back to the bottom paragraph, please, of the second page, the "for example" sentence. Does it refer, in part, to the ongoing work of your --
A Oh, so you're going back to the press release?
Q Yes, I'm sorry.
A Okay, sorry. At the bottom paragraph.
Q Yes.
A Oh yes, "for example", yes. Absolutely, yes, it does. We were in the process of looking at this.
Q As well as having published and already looked at some of the issues as it relates to libraries?
A We had made some libraries but we felt that the...

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The issue this was particularly addressing was the issue of -- if we look at, sort of, the challenge issued by Milstein in the form of this press release on the first page, the particular point was, "Can we make artificial human antibodies entirely outside the body without the use of human or animal immunisation?"
Now, the work that McCafferty describes is the display of a mouse antibody on the surface of phage and the selection of that. And obviously a variety of further steps would be required. For example, just making human antibodies; getting suitable PCR primers to amplify out the human V-genes, and to make sure that the way we did that didn't give us any undue biases.
Q And this was work that remained to be done?
A Well, this was work that either remained to be done or we were in the process of doing.
Remember the article had been submitted in August, and so a number of things would have happened between August and December when it came out. So, although I don't think we created diverse libraries by August, we may well

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have done by December.
Q What was the contribution, inventive contribution, made by David Chiswell to the application which ultimately became the 108 patent?
A To the best of my recollection, we regard, and certainly I regarded, that the invention was the demonstration of functional folded antibody on the surface of page, and that therefore the inventive contribution of David Chiswell came from his contribution to the joint effort that we had with McCafferty, myself, Andrew Griffiths. He was involved in making various practical suggestions and had certainly been very keen on the idea.
Q Who was it that first thought of the idea of displaying functional folded antibodies on the surface of phage, if you know?
A Who first thought of the idea? Well, I can't answer that definitely. All I can say is that the idea had been discussed by myself and Andrew Griffiths, probably some time in 1988 when he came to the laboratory.
We had revisited the idea some time in -- with actually much more urgency in the summer; I think probably about May or so, perhaps

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April/May of 1989. So all I knew is when we had discussed it. There may well be other people who may have had the idea.

Q This was before any association or affiliation with David Chiswell, am I right?

A Correct.

Q What was the urgency that you alluded to in the summer or May of 1989?

A The summer of '89?

Q My notes say -- they may be incorrect -- that you answered the question on when it was that the idea was addressed with more urgency --

A Oh sorry, yes. We started -- as I say, it was probably more like April or May. We were concerned...

What had happened is that, at that point, we had expressed antibody VH repertoires as secreted fragments from bacteria, and Andrew Griffiths had determined the sequence of many of the V-genes, and we saw there was a huge range of them. In other words, the sequences appeared to be, from the sample of fifty that we took, my recollection is that there were only a few that were the same, if any. I can't remember now

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exactly what it was. But we therefore felt that the PCR method that we'd taken was clearly generating many diverse genes.

And, at that point, we had undertaken -- we were undertaking work with the D1.3 VH single domain. Now, this is from an antibody which binds to lysozyme, and Sally Ward had shown that the VH D1.3, when this bound to -- that the VH D1.3 bound to lysozyme with quite good binding activity; in fact, almost as good as the -- I think it was the Fv fragment that she was using at that point. But when we combined that with other light chains, we found that we couldn't demonstrate binding. It's as though the addition of a light chain, that wasn't the actual original light chain, turned off the binding of the heavy chain.

So I felt that there must be light chains in there that would mimic in some way the original light chain of D1.3 antibody and would give us binding, and I was interested in that because we wanted to be able to move from single domains to expressing complete antibody fragments with the associated chains.

So the strategy at that point that

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we were considering was the idea of identifying heavy chains with binding activity by the methods where we'd -- by, essentially, ELISA-based techniques, and then trying to find a light chain that could be paired with those that would give one binding activity, even though it wasn't the original light chain. But it was clear that that frequency was going to be rather low.

It was also clear that other strategies which we had, where one might just throw the heavy and lights together at random, the frequencies there would also be extremely low, and we felt that the use of screening methods was really not the way to go.

And so, at that point, we wondered about methods of selection. And I had a discussion with Andrew Griffiths on perhaps the best way that we could use to select antibody fragments. One way would have been to express them on eukaryotic cells, and I think that that approach was inspired by some work by Brian Seed.

Another way was to try to express them on bacteria. And work by a French group had shown that peptides could be expressed on the surface of lamB, which is an E. coli protein, and

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I think it had been shown by binding of anti-serum that such bacteria could be selected or had been precipitated by anti-serum.

And the other possibly was the experiments -- was the use of bacterio phage using gene III.

Q And that's the work of George Smith?

A And that was the work of George Smith, correct.

So we had these possibilities in front of us.

Q An you chose ultimately to use the gene III bacterio phage rather than George Smith, correct?

A We ultimately chose to do it, although Andrew Griffiths did start some work with lamB, which was not successful.

Q Now, I'm going to return, if I may, to David Chiswell's contribution. First of all, let's go back in time. When was the first time that you ever met Dr Chiswell?

A I'm not absolutely sure. I was a consultant for Amersham. Amersham at that point were interested in humanising an antibody. In fact, I advised -- I don't know whether at that

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point or before that -- a number of companies that were interested in humanising antibodies. One of those company was, for example, Unilever. Another one was -- there was Amersham. I was advising Scotgen. And at one stage I was advising Celltech.

I was very interested in seeing this technology applied, and companies wanted to apply it, so I agreed to consult them for, I think -- I agreed to be a consultant for a very limited number of days a year. It might be one or two; I simply can't remember what it was.

And I think that, in Amersham, Chiswell was part of the group. I think he was the group leader of the people who were humanising antibodies at that time.

But, actually, I only remember meeting him I think twice in Amersham. There was a meeting I had where they were describing their project, but they didn't, in fact, tell me what the antibody was supposed to do. They were extremely secretive. In fact, I found them rather frustrating to advise because they revealed very little of what they were up to. Other companies were much more open. Because one had a confidentiality agreement, you know, I felt it was

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actually better if I had some idea of what they were actually after, and obviously I would keep it confidential, but Amersham told me nothing about what they were doing; whether it was for therapeutic or whether it was for diagnostic purposes, or imaging, or whatever. So in that, kind of, unpromising aspect, I actually...

Chiswell was the group leader of that, and so I was actually, naturally, a little bit hostile to the way they were treating me. But, in fact, Chiswell was very pleasant and I got on with him very well. You know, he managed to somehow overcome this intrinsic problem that Amersham had with not divulging anything.

Later on, at some point, and I don't know when it was, I met him in the States, but I have very dim memories of that.

Q Can you give us at least a time frame of these initial consults with Amersham during which you met David Chiswell?

A The very first I simply can't remember. We're probably talking about '88 or '89.

I did meet him in Amersham in the summer of '89 for a second time, or at least a

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second time.

Q Do you have specific recall of that meeting?

A Yes, I do have recall of that meeting.

Q What occurred at that meeting, to your recall?

A I had been asked to go and give a talk there. The intention had been to combine a consulting session with giving a talk to enthuse Amersham about the prospects for antibodies. But it turned out that, when I went there, that Amersham appeared to have made a decision to axe the antibody work, the humanising work. In fact, I gave a talk in Amersham about our work on VH repertoires and the possibilities of building up antibodies from such repertoires.

Q During this same session?

A During this same session. Well, what happened was that there had been a consulting session I was supposed to have, and then there was a talk after it -- or perhaps it was the other way round; I simple can't remember -- but I never got the consulting session because they just said, "We've all lost our jobs or we're going to be

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shifted somewhere else."

Q Did you have a conversation with Dr Chiswell during this session in the summer of 1989 about any plans he had, if any, with reference to starting up a new company?

A We touched on it. He actually was clearly very excited by the new technology I described, but also clearly had been shocked by the fact that his whole team was being axed, and I believe relocated to Cardiff, which is in Wales.

Q Dr Chiswell didn't go to Cardiff, to your understanding, did he?

A No, he didn't go to Cardiff, no.

Q I'm sorry, finish your answer.

A Sorry. I had a brief discussion with him and I think I had a brief discussion -- at least I did have a brief discussion at that stage with Peter Garland, who later became the chairman of Cambridge Antibody. Peter Garland was, at least, I think was heading the research side of Amersham. So I remember meeting Garland for the first time at that time.

Q Did either Chiswell or Garland express to you their desire to start a new company that would work in the antibody field?

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A I think Chiswell told me that he was interested in starting a new company, and that obviously humanising would be one approach. But another approach might be the development of this more recent technology that I described, and he was very enthusiastic about that.
Q Did he say anything to you about an idea that he had about attempting to display antibodies or antibody fragments on bacterio phage?
A No.
Q At that time?
A No, he didn't.
Q Did you have any discussion during this session with anyone in the summer of 1989 concerning such an idea? And now I'm referring to your Amersham context.
A No.
MR SKILTON: Let's see, it's quarter to 11. Why don't we take our first break?
VIDEOGRAPHER: We're going off the record. The time is 9(sic).41.
(A short recess at 10.41 am)
(Resumed at 11.00 am)
(Exhibit 8 marked for identification)
VIDEOGRAPHER: We're back on the

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record. The time is 10(sic).00.
BY MR SKILTON:
Q Dr Winter, I have placed in front of you what has been marked as exhibit 8 in these proceedings, which is a document which purports to be an invited participants' list with respect to a certain conference, and bears the Bates stamp numbers of MO81881 through 1883. Is this exhibit 8 --
A Sorry, exhibit 8, yes. It says at the top exhibit 4, but at the bottom it says exhibit 8.
Q The reason this is in front of you is that if you look at the last page you'll see, under W, which is -- I think you're condemned for life to be -- on the last page, Winter. Usually I make it to the penultimate page. And invited participants is the list.
Do you recall whether or not you went to this particular conference?
A Yes, I did not.
Q Do you keep any kind of a calendar to indicate to you what conferences you do attend in any given year?
A Not really, no. But I can say,

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quite categorically, I did not attend this conference.
Q And that's based on a certain recollection?
A It's based on the recollection I was one of the organisers of this conference and I did not go to it.
Q Did anyone, to your knowledge, from the MRC go to this conference?
A I can't be sure of that. For example, Sydney Brenner, who is marked on this first page, as MRC Molecular Genetics Unit, he may well have gone to the conference. I believe Cyrus Chothia did not. I believe Andrew Griffiths did not. I believe Terence Rabbitts did not.
Q Are you aware of --
MR VEZEAU: Excuse me. (To the witness) Are you finished with your answer.
MR SKILTON: I'm sorry.
THE WITNESS: Yes. There are no more MR -- oh, sorry, and Sally Ward did not.
BY MR SKILTON:
Q Was there a reason that you did not attend this conference?
A Yes. I did not wish to attend the

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conference.
Q Why was that?
A I didn't wish to attend the conference because I didn't think it was in the MRC group's scientific interest, that's my group's interest, to attend the conference and discuss our ideas at a time of great scientific competition.
Q What was the great scientific competition that you've just referred to, at this particular time, April, to be specific, 23 through 26 of 1990?
A Well, that's the date of the conference, but the date of this list is November 27, '89.
Q Okay.
A So I think it was actually -- I can't remember when I decided that I wouldn't go to the conference, but I think it was either in very early '90 or very late '89.
Q What was the scientific competition? I think you used the word great scientific competition?
A Correct. Between my own group and that of Richard Lerner's.
Q And that was the technology you

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alluded to earlier in the day?
A To do with antibody repertoires, correct.
Q What was the risk or jeopardy that you were concerned about should your group, or any member of it, attend this conference?
A There were a number of...
As I understand it, this conference had been intended to be -- because it was invited participants, was intended to be confidential. And Richard Lerner, because of, I think, the funding by SmithKline, this conference, there were at least -- I can see here, actually, there were two people; Christoffersen and also George Poste. I don't specifically remember those people as being invited, although they are on this list, but nevertheless I felt -- oh, yes, there's three; there's Rosenberg from SmithKline.
And I actually felt quite uneasy about the presence of those people from a major pharma company, essentially getting a privileged window into a series of discussions that were going on between some of the key protagonists in the field.
Q Prior to July 10 of 1990 did you go,

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personally go, to any conference that talked about or had as a subject of discussion the phage display of antibodies or antibody fragments?
A Before 1990? I don't remember any conference that I went to before July 1990 where that was a topic of discussion.
Actually, I don't think I went to any conferences at that point. I decided that the work such sufficiently sensitive that we should just shut up and get on with it.
Q Was that a policy, then, of your laboratory?
A It was a policy of my laboratory at that time.
Q And at that time now -- and I'm being specific on the date -- let's say from November of '89 through July of 1990, is that the time frame that you are referring to in your last answer?
A Yes. We were very careful about what was said in public.
Q Was it true that you believed that, though these conferences were "confidential", on occasion there was risk that either trade secrets or matters of confidence in the laboratory would be

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disclosed to an unauthorised person?
A There was certainly a risk of that. The problem I faced was that, if I go to a conference, I do like to be open, and there didn't seem much point in going to a conference on a new and developing technology where I'd be having to watch everything I said.
Q And that technology, as described here, is vectors for cloning the immune response. That's your understanding of what the subject-matter of that conference was?
A Although I can't actually remember exactly why the conference had that title, I think the title was a title suggested by Richard Lerner. He was the co-organiser with me.
Q For this record, would you define your understanding of the concept of "vectors" as used in this particular title?
MR VEZEAU: Objection. Lack of foundation.
BY MR SKILTON:
Q I'm asking for your understanding of it.
MR VEZEAU: Same objection.
BY MR SKILTON:

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Q Do you have an understanding of it?
A Of what the word "vectors" means?
Q In this context?
A Well, I'm looking at this title now and wondering actually why it had such a strange title. I mean, you might say I was an organiser of it, and I'm surprised that it has that title, but I think that, at that stage, there was -- well, almost certainly, Richard Lerner was very concerned about his use of Lambda phage, which he believed gave him a massive advantage over other systems for screening, direct screening of antibody fragments with binding activities. And I think that part of his interest was to...
Well, so Richard had become interested in the vectors, the kind of systems you'd use, and the Lambda bacterio phage gave them a way of being able to create very large combinatorial libraries by essentially cloning each library on different parts of the bacterio phage and then putting them together.
So, whereas I think previously Richard Lerner didn't know very much about vectors, he was obviously very excited by his results and the technology that he had, and was, kind of,

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trying to get people to use the Lambda bacterio phage system.
Q Let's get a time frame more specific in reference to the conference date of April, late April of 1990.
Your lab, by that time, had undertaken its work with bacterio phage and the display of antibody fragments, is that correct?
A I believe that we had displayed and demonstrated binding activities of antibody fragments by the date of that conference.
Q Had McCafferty begun his work, if you recall?
A Well McCafferty -- I told you earlier, McCafferty started in January 1990. I think that's on the record.
Q Right. And I believe his lab book shows as its first entry January 22nd of 1990. Is that a date, as you sit here, that seems sensible to you in terms of when he commenced his work?
A I actually thought he started before that but, of course, it may well be that he was getting reagents sorted out. But I think he was physically present in the lab when we were discussing the work well before 20th January.

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Q In reference to the word "vectors", how would you describe what he was doing with reference to that concept of vectors?
MR VEZEAU: I'm going to object to that question as indefinite and lacking any foundation.
BY MR SKILTON:
Q What vector was he experimenting with?
A As I explained earlier, McCafferty was experimenting with a filamentous bacterio phage vector, so it was very distinct from the Lerner Lambda bacterio phage.
Q Do you know whether or not any other lab was aware of your choice, your lab's choice, of the filamentous bacterio phage vector in the period from January 1st 1990 through April of 1990?
A I don't believe that anybody outside my own group knew that we were experimenting on bacterio phage at that point.
Q Did you say you don't believe?
A I don't believe anybody outside knew that we were experimenting on bacterio phage.
Q Was this a matter that you were, as head of this group and this lab, most concerned

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about keeping confidential during this period?
A There were two aspects of keeping confidential. The first was to do with the scientific competition. But the other one was an understanding that I had with David Chiswell, and in fact the backers of Cambridge Antibody, PepTech, that we would make sure that we kept our work confidential so as not to prejudice any commercial filings.
Q And that was part of the concern of your personal decision not to attend this conference, is that correct?
A It's difficult for me to remember all the concerns I had, but there were several, as I've explained. There was the privileged position of SmithKline in the conference, who appeared to have provided money for drinks and perhaps was subsidising the costs of the conference, but really had not done any work. The people involved, as far as I was aware, had done no work in the particular field we were talking about, and it seemed as though, therefore, SmithKline was getting privileged access, commercial privileged access, to a body of academics.
Q In any event, was it also another

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concern that you did not want to disclose to any other business entity the particular work of your laboratory with respect to the bacterio phage vector at that time?
A Well, there would have been no obligation for me to disclose it, even if I had attended the conference. I simply could have dealt with other work that was going on in the laboratory.
But my particular reason for not going was I saw no point in going to a meeting, which was supposed to be open and free exchange of ideas, and then keeping a number of ideas totally secret to myself.
Q Now, you mentioned Dr Chiswell's position. Let me be specific in terms of some of the players here. Let's talk about the period of January of 1990 and Dr McCafferty.
Dr McCafferty had been at Amersham, is that correct, to your understanding?
A Yes.
Q And he came from Amersham to CAT, is that your understanding?
A He came to my laboratory.
Q What was --

A From --
Q I'm sorry.
A From Amersham to my laboratory. You have to recognise, in those days CAT didn't have a physical existence, apart from in the form of the people and a bank account and Articles of Association. There wasn't a laboratory for Cambridge Antibody then.
Q Was there a contractual relationship, then, that Dr McCafferty had with your laboratory during this time that CAT had no physical existence?
A A contractual relationship that McCafferty had with the laboratory?
Q Yes.
A McCafferty was a visitor to the laboratory. He was employed by Cambridge Antibody Technology, but he was visiting the laboratory in order to work on projects which had my approval, and he was accepted in the same way as we'd accept an academic visitor.
Q Do you know who paid his salary during this period?
A I believe Cambridge Antibody paid his salary.

Q Was there an arrangement at that time concerning any inventive activities that he might undertake during this period and who would get the benefit of them as between MRC and CAT?
A There was.
Q What was that arrangement?
A The arrangement had been discussed before McCafferty came into the laboratory. Obviously there were these two pressures. Cambridge Antibody had no laboratory. They also wanted to get involved with some aspect of repertoire technology, but had nobody familiar with it, and so part of the idea was that McCafferty would be trained in my laboratory in this field. So that was the interest from Cambridge Antibody's side.

I, at that stage, had a relatively small group, and I needed to try out a variety of ideas for looking at the repertoire technology; either looking at methods of screening or methods of selection. So, therefore, from my point of view, there was a genuine scientific interest in getting someone in who had, even though he wasn't formally trained in the field, could probably be trained up fairly readily.

So those were the interests of the different parties.

Now, at that stage, I had a central role in the founding of Cambridge Antibody Technology because I was the link to PepTech, who were the financial backers.

PepTech were also very keen to get on with the work, but obviously were concerned that any work that might take place in my laboratory, unless it was specified, that such work might simply just be taken by the MRC.

Now, equally well, Aaron Clug, who was then the director of the laboratory, was concerned that the rights of the MRC were represented, and that, essentially, ideas generated in the laboratory just didn't simply become the property of Cambridge Antibody or compromised by Cambridge Antibody Technology.

At that time I, therefore, had a discussion with Geoff Grigg and with David Chiswell, representing the Cambridge Antibody/PepTech side. They both were quite keen that McCafferty could come in to the laboratory and get started.

I had a discussion with Aaron Clug

about how this might be arranged. And it was agreed that, basically, between us -- and I specifically asked Aaron this and he confirmed it to me, and probably also directly to David Chiswell and Jeff Grigg -- that work that originated from McCafferty's activities would belong to the MRC and Cambridge Antibody Technology jointly, which would mean, in the worst possible case, that, if we didn't come to an agreement on other aspects -- for example, that there was no agreement at that time to give Cambridge Antibody the rights to use the repertoire technology; that was still owned by the MRC -- so at that point it was agreed that, anything emerging from McCafferty's work, would jointly belong to the two.

So the MRC, essentially, at that point, didn't guarantee to make available the repertoire technology to Cambridge Antibody. I think that occurred some time later. Equally well, Cambridge Antibody knew that it couldn't be cut out of anything that McCafferty discovered.

Q Was there a document that reflected or stated this relationship, that you're aware of, during this period?
A I'm not aware of a document that

states that. I mean, you have to remember, I mean we were working in a hurry, all the parties trusted each other, and, as far as the MRC was concerned, they felt very comfortable with the arrangement, because they had no guarantee on making available the repertoire technology which they thought was obviously going to be part of Cambridge Antibody's work.

PepTech felt less comfortable but, nevertheless, in the interests of getting going, they agreed to it, believing it would be possible to come to an agreement; which, in fact, is what happened.

Q The PepTech person was Geoff Grigg?

A I believe he was the main person involved in those discussions.

Q Would you describe this, generically, as a handshake type of an arrangement? Does that make sense to you to call it that?

A It's a handshake arrangement between people who'd known each other for a long time and trusted each other.

Q You've known Jeff Grigg for a long time?

A Yes, I have.

Q Or you did at that time as well? Had known him?

A I had known him -- I had known him, I think, probably in the seventies and then again in the eighties, early eighties.

Q By reason of your -- was it a consulting relationship that you had with PepTech?

A No, I didn't have a consulting arrangement with PepTech. It was to do with...

Geoff Grigg was actually at the laboratory, at the MRC laboratory, working with Fred Sanger, when I first became a student there. And I met him, but I didn't get to know him very well at that point.

Later on, he had a brief sabbatical of a few months in the laboratory, and by that stage, I can't remember exactly how, I did get to know him, and I got on very well with him.

The early work that I was involved in doing with influenza virus involved two people from the laboratory going to Geoff Grigg's laboratory at North Ryde in Sydney. There was a post-doctoral worker I worked with -- sorry, he was then a PhD student -- who was Stan Fields. And

he went across to Australia and he'd come very much to like Geoff Grigg. And also George Brownlee, who was my immediate boss when I was working on the influenza virus, had been across there. So there were contacts between the laboratory in a general sense, and people I was associated with, that -- I've forgotten how I started the sentence.

But, basically, there was knowledge on both sides of the other side. We'd sent people to Australia, and Geoff Grigg came across to the UK. And so, in the early eighties -- I think it was the early; perhaps it was later eighties -- he came into my laboratory, working in a fume cupboard at that stage. It was the only space we had available, was a fume cupboard.

Q You mentioned there was no agreement as to the repertoire technology at that time. There was a later agreement?

A There was a later agreement, I believe, on the repertoire technology.

Q And I'll get to that, but I want to keep your attention, please, on this first window.

A Let's put it this way; there was no agreement, but there was an understanding that...

Of course, at that point we didn't

know what was going to materialise from McCafferty's work, and Cambridge Antibody had hopes of being able to utilise anything emerging from McCafferty's work, in combination with the basic repertoire technology.

From my soundings in the MRC, and I think particularly Sir Aaron who ran the laboratory, the kind of green light was given that, if everything went well and if the company was successful, then there shouldn't be a problem in making that available, at least on a non-exclusive basis.

Whether or not that repertoire technology would be made available on an exclusive basis, had been raised, but certainly no understanding had been reached on that.

Q And the basic repertoire technology, as I understand your answer, had already been developed? You earlier indicated that, I believe?

A Correct.

Q In fact, you had published on that technology?

A We had published both as a scientific paper and we'd also filed a patent application on it.

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Q This technology was, if you will, the battle between the Lerner group and the Winter group?
MR VEZEAU: Objection. I'm not certain what you mean by "this".
MR SKILTON: The repertoire technology.
THE WITNESS: Was the battle. Well, there essentially was scientific competition. I wouldn't refer to it as a battle, because it was more like a war, actually; a series of battles over a period of time in which we realised that, unfortunately, we were going to be in competition with this very large group in the States.
BY MR SKILTON:
Q Perhaps inartfully, and maybe not even correctly, but can this technology also be described as the library technology?
A If you want to do so, yes. If you're referring to the basic aspects of it, yes.
Q So when they came here, you hadn't yet reached an accommodation --
A Sorry, when who came here?
Q Let me try again.
A Sorry.

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Q You're fair in asking for a little more precision in the question.
When Dr McCafferty came to your lab in January of 1990, as I understand it, there hadn't been yet a resolution as to whether or not CAT would be given access to this basic repertoire technology?
A There was not an understanding -- there was no agreement. There was an understanding that, if things worked out, they should get non-exclusive access to it, at least.
MR SKILTON: Let's change the tape.
VIDEOGRAPHER: This marks the end of videotape number 1 in the deposition of Dr Greg Winter. We're going off the record. The time is 10(sic).29.
(A short recess at 11.29 am)
(Resumed at 11.30 am)
VIDEOGRAPHER: This marks the beginning of tape number 2 in the deposition of Dr Greg Winter. We're on the record. The time is 10(sic).30.
BY MR SKILTON:
Q We concentrated the last series of questions on Dr McCafferty.

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MR VEZEAU: For the record, the time is 11.30. I understand on your tape it says 10.30.
VIDEOGRAPHER: I did say so at the beginning.
MR VEZEAU: Yes, thank you. I'm sorry, go ahead, John.
MR SKILTON: Okay, let me try again.
BY MR SKILTON:
Q We've been concentrating in the last series of questions on Dr McCafferty's role, initial role in the MRC lab.
Were there others from Amersham who came and worked in your lab during the same time window, the early period of January, February and March of 1990?
A I don't remember anybody.
Q Did Dr Chiswell at any time work in your lab during this time frame?
A It depends what you mean by "work". He came to visit the laboratory and we had discussions together, but he was not involved in any practical work.
Q Did Dr Chiswell bring with him the idea of displaying antibody or antibody fragments on the bacterio phage vector?

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A As I understand it, Dr Chiswell had had this idea some time during the summer of '89, and so, therefore, he'd had the idea independently from us.
Q How did you find that out?
A I found out shortly before McCafferty started. Again, we were having to discuss the appropriate projects to do for John, and there was obviously the possibility of screening methods. There was also the possibility of various selection methods. So we had a discussion of those. And it turned out that, during that discussion, that Chiswell actually stated that he was very keen on the idea of using the phage, given that our lamB work, at that point, didn't seem to be working out.
Q And did he tell you how it was that he came upon the idea of using the bacterio phage vector?
A At that stage he didn't.
Q Did you find out at some stage?
A I found out later on that the idea had originated in a discussion that he had between Tony Pope and himself. And I think I found that out before the filing, the final filing in 1991.

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Q Now you're referring again, I assume, to the filing that was the first priority filing, that ultimately resulted in the 108 patent?
A Sorry, '91 would have been the filing -- it's a year after the first priority filing. The first priority filing was July 1990. I'm referring to July '91.
So I was not aware of exactly how the idea had come to Chiswell and McCafferty; at the point when McCafferty came to do the work or, indeed, at the time of the first filing.
Q You were aware that Chiswell had had the idea?
A Well, not really. I was aware -- I was aware, let's put it this way, that clearly Chiswell, since I had not told Chiswell directly that we were in the process of undertaking selections with lamB, and indeed had considered phage, he appeared to have come up with the idea independently. So I didn't actually, at that stage, you know, subject him to a Spanish inquisition as to exactly how the idea had originated. I had assumed...
He referred actually to "we" and, by that, I had thought he had meant John McCafferty

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and himself. But, if I go back, I simply can't remember. I can't remember the details. But certainly I had the impression, at that stage, that it was John McCafferty himself. He certainly never said "I" at that point.
Q But then he didn't say Tony Pope at that point?
A He did not say Tony Pope at that point.
The problem really was that, at that stage, we were playing with lots of ideas and we really just didn't know whether that idea was going to work. So, later on, it assumed some significance. You can have people coming along and saying, "Oh, yes, I had that particular idea" when other people have gone to the significant trouble of demonstrating it. And at that point, you know, it was just another of the ideas that we wanted to play with. And, in fact, we were also interested in the idea of developing a screening system.
Q In your last answer the "we"s referred to MRC and your laboratory? Do you understand what I just asked you?
A Not really.
Q Let me try again.

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A I was referring to "we" in the context of Chiswell.
Q Let me see if I can... Again, as much precision at your recall permits here.
Before McCafferty actually undertook his work in January of 1990 directed at using the bacterio phage vector for displaying antibodies or antibody fragments, you had a conversation with Dr Chiswell concerning the idea of using such a vector. Is that a correct statement?
A Before McCafferty started work, I had a discussion with both of them. I can't remember whether it was at the same time or separately.
Q During this discussion, and prior to the commencement of the work, Dr Chiswell told you that he had the idea of using the bacterio phage vector?
A No. As I think I told you, he didn't say, "I have the idea of using the bacterio phage vector." He said, "We've had this idea that we should try." Or, "What about this idea?" It was something of that sort. You know, it was...
I mean, when I thought back to it, you know, I can't claim that he used the word

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"I" at all. In fact, for some reason, I had had the idea that because he used the word "we" or "We've had an idea", or something like that, I think he was referring to himself and McCafferty, but now I realise it may well have been Tony Pope.
Q Did you respond to him, at that time, that you had had a similar or the same idea?
A At that time I did, yes.
Q Can you recall, Dr Winter, what it is exactly that you said to him?
A No.
Q It was words to that effect?
A It was.
Q Did you reach agreement that, as a result of this conversation, that this is a project, then, that Dr McCafferty should explore experimentally?
A Yes, we did. There were two projects. I think the other project was screening; I think linking an enzyme to antibody single domains, but I can't be sure of that.
Certainly there were at least two projects which McCafferty was involved with, and that was one of them, and I gave my approval that we should go ahead with that project.

Q Was this a project that Chiswell had recommended to you, based on the fact that he had had this idea for McCafferty?
A I'm sorry, could you repeat that?
Q I'll try to rephrase it.
A Yes, sorry.
Q Did Chiswell come with a recommendation to you that McCafferty do this work with respect to the bacterio phage vector?
A I don't remember it quite in that form. I remember that it was more on the lines about, "What about this idea?" And when we said, "Well, actually, we've been thinking along similar lines and, in fact, Andrew Griffiths has been trying lamB, that an alternative might well be filamentous bacterio phage", I think we both decided this would be a great idea.
Q Did you have any such conversation with Dr McCafferty himself, if you recall, at this time?
A Obviously I had a discussion with McCafferty about what he was doing, or what he would be doing, but I simply can't remember how that came about.
You know, there were meetings which

we had together, certainly in the early... In either very late '89 or very early 1990 there were meetings that we had together and so, you know...
It wasn't the case that McCafferty -- that, in other words, Chiswell and I simply said, "Right, this is a good project. It's good commercially. It's good scientifically. Go to it, boy." I mean, it wasn't anything like that.
With any scientist coming to work in the laboratory, you have to take them on board and discuss the idea. Certainly McCafferty had discussed it with Chiswell, I believe before Chiswell raised it with me, because he used the term "we", and I think McCafferty was there at that stage.
Q Did McCafferty, at any time prior to commencing the work, tell you that he had actually already done some experimental work with respect to this project?
A He didn't. In fact, I wasn't aware of that. I'm not even aware of it now.
Q I'm going to mash a word here, but hopefully it will permit you to answer the question.
"Oligonucleotides", does that have

any meaning in reference to the experimental work that McCafferty eventually undertook with respect to the bacterio phage vector?
A In cloning an antibody gene into the bacterio phage, my recollection is that we needed to change the restriction sites in the vector, and I believe that McCafferty used synthetical oligonucleotides for that purpose, but I wasn't aware that he had done that before he came.
Q Did you ever hear anything like that at any time?
A No. You'll probably tell me it's in McCafferty's testimony but, in fact, I didn't notice it, or I didn't read the testimony like that.
MR SKILTON: Number 9.
(Exhibit 9 marked for identification)
BY MR SKILTON:
Q Dr Winter, I'm placing in front of you a document which has been marked as exhibit 9 in these proceedings. It bears the Bates stamp number of CM040142 and proceeds through CM040146.
First question, have you ever seen this document before?
A Just give me a chance to read it and

I'll try and tell you.
Q Indeed.
A (Witness reviewed the document)
Yes, I don't specifically remember this document, but I do remember one of the points that was raised in the document which I didn't agree with.
Q What point is that, please?
A It's on 040144, and it's the point relating to intellectual property, the point that says, "Greg and I have discussed the history with Sir Aaron and have agreed that the invention was by John and myself..." So that point I did not agree with and I don't believe it's correct.
Q When was the -- strike that.
Have you ever seen this particular document before today?
A I can't be sure of that. I think I may have seen part of it recently. I mean, the lawyers threw a number of things over the desk when I was talking to them, but I don't specifically remember that it was exactly this document. I think there may have been this page, but there may have been the others as well. I'm not sure.
Q In any event, the sentence you've just referred to is one that was brought to your

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attention prior to this deposition?
MR VEZEAU: Objection. I think now, the way that question is phrased, we're into work product certainly and privileged communications, and I will advise you, Dr Winter, that you need not answer that question on that basis.
MR SKILTON: I disagree with that instruction. I think I have a foundation to enquire. He has indicated he reviewed a portion, he believes, of at least a portion of this document in preparing for this examination.
BY MR SKILTON:
Q Is it your testimony that you read this sentence in preparing for your deposition, Dr Winter?
A As I understand it, my attorney is advising me that I should not reply to that.
MR VEZEAU: This is a different question.
THE WITNESS: Oh, I see.
MR VEZEAU: So listen to this question.
(To the court reporter) Thelma, if you would please repeat it?
COURT REPORTER: "Q. Is it your

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testimony that you read this sentence in preparing for your deposition, Dr Winter?"
THE WITNESS: Yes, I did.
BY MR SKILTON:
Q Was that the first time, to your best belief and knowledge, that you ever read that sentence?
A I can't be sure. What I remember is that, during the early discussions with the MRC, that Chiswell represented that the invention was by John and himself, and this was something that we did not agree with. But whether I saw it in a document at that time... Actually, I don't think I did, or I think I may have seen this for the first time. But I was aware of that sentiment, and I was also aware that we actually -- that the MRC didn't accept that.
Indeed, when we had further discussion about it in defining what was the invention, the contributions made by individuals, that we decided that that was not sustainable, and I think that was a point in which Chiswell agreed.
Q Let me break it down a little bit here.
Is it your statement that the

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sentence that reads, and I'll read it again in the record: "Greg and I have discussed the history with Sir Aaron and have agreed that the invention was by John and myself...", and then it continues, that clause is an inaccurate clause?
A I believe, from what I remember, that indeed I did discuss the history with Sir Aaron, but I can't remember whether I discussed it with Chiswell or whether I discussed it separately. I did believe I discussed it actually separately with Sir Aaron. So I represented, or at least I had a discussion with Sir Aaron. I believe Chiswell had a discussion with Sir Aaron. But I certainly had not agreed that "the invention was by John and myself".
At the early stages, one of the points that Chiswell raised was whether the insertion point, the precise insertion point in the vector was a key aspect of the invention, and that one of the points he asked me is, "Well, do you agree that, if that first insertion point" -- I've forgotten what it was they'd taken or whether they had gone two residues in from the end terminus; I can't actually exactly remember where the fusion was -- "if that exact fusion point was indeed a key

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aspect of the invention, then didn't I agree that the invention was essentially by John and himself since they had chosen that insertion point?" And I had to accept actually that the choice of the insertion point I had believed was made by John McCafferty.
So if it had been that the exact insertion point was the invention then, in this kind of rather rambling discussion I had with Dave where we were trying to identify what the key features of the invention were, then it might be the case the invention was by John, and possibly Dave to the extent that he may have been involved in agreeing the sequence of the oligonucleotide to make that insertion.
But actually I didn't have the same view of the patent. I actually felt that, and always did feel at the very beginning, that the patent was about the demonstration of folding, the production of folded antibody on the surface of phage, and the ability to use phage as a selection vehicle. In fact, that was a view which both John and Dave agreed with me, after some early discussions.
My belief about this statement is it

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was more to do with Chiswell who, at this point, is negotiating commercially with the MRC. He's negotiating with Martin Wood -- I think it's Martin Wood; it looks like his writing -- of MRC technology transfer, and I believe that he represented what he saw as the best case for Cambridge Antibody.

Q By a representation, nevertheless, with which you disagreed?

A It's a representation -- it's a representation I did disagree with at the time. I made it clear I disagreed. And the only circumstance in which I might have agreed it, certainly as a scientist but not as a patent lawyer, would have related to the idea that the exact insertion point was central to the invention and no other insertion point would work. So that was the discussion that we had.

At that stage I think, in fact, David Chiswell did believe, transiently, that that was a key feature of the invention, but I don't think that this is...

I think this whole sentence is contradictory, and I think has to be seen as a commercial document, the process of negotiation

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with other people in the MRC, and certainly not been passed over. I'm fairly sure it wouldn't necessarily be accepted that I would see it. In fact, I don't think I did see it.

Q In fact, you believe that it is false that you ever agreed that "the invention was by John and myself", don't you?

MR VEZEAU: I'm going to object to that because I think that question has been asked and answered.

MR SKILTON: I don't think it's been directly answered, and I ask that it be answered.

MR VEZEAU: Well, I mean, you're characterising something as false, and I think the witness has explained different views and, in that sense, I do believe it's not a question of falsity. I believe it's a question that's been asked and answered in quite some detail.

MR SKILTON: You have made your objection. You need not argue answers.

MR VEZEAU: Thanks.

MR SKILTON: Would you answer the question, please, Dr Winter?

THE WITNESS: Can you repeat the question, please?

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MR SKILTON: (To the court reporter) Please.

THE WITNESS: I've lost track of it.

MR VEZEAU: Thelma, would you read it back, please?

COURT REPORTER: Certainly.

"Q. In fact, you believe that it is false that you ever agreed that 'the invention was by John and myself', don't you?"

THE WITNESS: I've explained to you what I believe the invention was and that, therefore, with that view of the invention, this statement is false.

BY MR SKILTON:

Q Now, you indicated that you might have seen pages of this document. Let's go back and identify a little bit some of the players. If you'll go to exhibit 9, the first page?

You earlier stated who Martin Wood is. What was Martin Wood's position at that time with the MRC?

A I don't know the formal title of his job, but he was in the technology transfer office at MRC head office.

Q Is it correct that, as of the date

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of July 30th 1990, as you read the document, there was not yet any formal agreement -- by that I mean a written agreement -- in place between MRC and CAT?

A I don't believe there was any written agreement in place. Well, hang on.

Q I'm sorry.

A Sorry. Yes, I don't believe there was a written agreement in place between Cambridge Antibody and the MRC. However, there was, before John McCafferty came into the laboratory, there was a document he signed, which is the MRC visitors' form, which actually touched on any rights that he himself might have in respect of inventions arising from the work he was doing. So in that indirect sense there was something, but I don't believe there was a formal agreement between MRC.

Q The document -- and I'm going to go through it more thoroughly with you. That's a warning Dr Winter, in the sense if you want to read it over the noon hour to be prepared on it, that's your right to do that.

But it talks about a form Y in this document. Is that what you were just alluding to?

A I believe it's form Y, yes.

Q This is a form he would have signed and did sign, McCafferty?
A Yes, he did sign it.
Q Have you ever seen that signed form?
A I must have seen it a long time ago, yes.
MR SKILTON: On this record, I would request that signed form.
BY MR SKILTON:
Q Now, Diana Dunstan was whom at that time?
A I think at that stage she was the head of MRC administration at the laboratory of molecular biology.
Q Do you recognise the handwriting on page 143, the second page of this document, at the bottom?
A I said earlier I thought that was Martin Wood's handwriting.
Q All right.
A It looks familiar, but I couldn't be sure of it.
MR SKILTON: I am not able to fully read the script. Some of it seems to have been either cut off in copying or simply a bad copy of a

document.
Counsel, is there an original of this document available to Dr Winter and myself?
MR VEZEAU: I don't think available. What you have appears to correspond to the best copy we have.
MR SKILTON: I will try to read it into the record, but I do not purport to be an expert in this script.
MR VEZEAU: Nor would we agree with your characterisation of the text.
MR SKILTON: Well, I'm not trying to characterise it. I'm trying to get it read into the records.
Instead of having me do it, let me ask the witness.
BY MR SKILTON:
Q Dr Winter, are you able to read what is said at the bottom here?
A Not with certainty, no. Obviously bits I can read. Other bits I might be able to make sense of if I thought about it, but the most straightforward would be to get Martin Wood, if he's the author of it, to read his own writing.
MR SKILTON: I will ponder it over

then myself, and not waste the record time doing it.
THE WITNESS: Okay.
BY MR SKILTON:
Q Look then to page 144, the third page of the document, CMO40144.
A Yes.
Q Under item 2.1, history, did you review that in preparing for today's deposition, that statement?
A To the extent I believe I saw this page, I have looked at it. I wouldn't say I've reviewed it or thought a great deal about it.
Q Let me read it so there's no confusion as to what my reference point is. I'm not going to read the whole. I'm going to read the first two sentences into the record, and I'll ask you a question after each sentence respectively.
Do you understand this document to have been authored by Dr Chiswell?
A Well, to the extent that I have not seen it before, and to the extent that it has Cambridge Antibody Technology LT on the top, and appears to be a letter from Chiswell to Martin Wood, then, yes, I do assume he's the author. But,

as I say, I can't vouch for that.
Q Then we'll have Dr Chiswell vouch for it tomorrow.
A Okay.
Q But let's assume, you and I, that he is the author, and, if we're wrong, we'll be corrected in due course.
A Okay.
Q "Many people in the field, including independently Greg and myself, realised that expressing antibodies on the surface of bacteria or phage would be attractive."
Does that, to you, appear to be an accurate statement of the situation as it existed prior to Dr McCafferty beginning the work in his laboratory? In your laboratory, I'm sorry.
A Well, I wasn't aware that many people in the field were realising expressing antibodies on the surface of bacteria or phage would be attractive. I'd have to say I wasn't aware of many people who had even expressed that view.
Q Were you aware of any persons, other than yourself, or laboratories other than yourself and Dr Winter(sic)?

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MR VEZEAU: This is Dr Winter.
BY MR SKILTON:
Q I'm sorry; Dr Chiswell. I'll rephrase the question.
Were you aware of any laboratories or people, other than yourself and Dr Chiswell, who realised this?
A That expressing antibodies on the surface of phage or bacteria would be attractive? Yes, I am. I remember only one person --
Q Who was that?
A -- who made any expression of this view, which was a person called Jannie Cesarini.
Q Could you spell his name, please?
A No, it's Italian. It's something like C-e-s-a-r-i-n-i. Something like that.
Q How was it that you knew he was interested?
A Because he contacted me.
Q When was this?
A I believe this was some time in January or February of 1990.
Q Did he explain why he was contacting you then?
A I can't remember exactly. I do

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remember that he asked me whether I thought it would be a good idea to try to get -- because he'd seen the work by Lerner, and he'd seen our work, and he asked did I think it would be a good idea to try to get antibodies expressed on the surface of phage.
Q When you say "our work", are you now talking about your work in repertoire or libraries?
A The work on repertoires and libraries, yes.
Q Did he tell you how it was that he had come to this idea or realisation?
A I don't think so, no. I think he said he thought it would be a good idea. Jannie Cesarini had worked in the MRC laboratories some years' earlier and had been involved in Lambda expression.
Q Was he with a particular company or laboratory at this time?
A He was with a laboratory. I can't remember whether, at that stage, he was attached to a company or not, or if he ever had. I think he had an association with an Italian group called IRBM but I don't know whether he was employed by them or not. He was based in Italy at that stage.

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He'd worked in the MRC laboratories some years' earlier and then, subsequently, was making his career in Italy.
Q How did you respond to him in this period, roughly January of 1990?
A I said it was a good idea.
Q Do you know whether or not he was pursuing it, independent of you?
A I don't think he had pursued it independently of me. Well, at that point. I mean, he didn't seem to have any results. He was just asking whether I thought it was a good idea, and I said, yes, I thought it was a good idea.
Q Is it correct, then, that other than Jannie Cesarini you were unaware of any other person who had realised that expressing antibodies on the surface of bacteria or phage would be attractive?
A Difficult to be sure. I mean, I think, because I had been discussing things with Cesar Milstein, it was also clear that he thought it was a great idea. But in terms of people externally, I wasn't aware. I don't remember, at the moment, anyone else who I was aware of who thought it would be worth trying.

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You know, obviously, if we look at Milstein; he was an expert in the field of antibodies. Look at Cesarini; he was an expert in the field of bacterial genetics. So these were people who -- and they both had -- obviously Milstein is in the lab, and Cesarini had been in the laboratory, so this was, kind of, rather a discussion of what you might say is, sort of, laboratory ideas, and I felt I couldn't unfortunately at that point be open to him.
I felt rather embarrassed in dealing with Cesarini because I would have liked to have said, "Well, we're working on it." But, in fact, because of the agreement with Dave Chiswell, I couldn't.
Q In the next sentence, in context, again under 2.1 reads: "The problem has been how to make it work."
Does that sentence today have any meaning to you in reference to the context in which it was written in this document?
A I think the context in which it was written was that, as far as Dave was concerned, at that time when he was writing this document, that the point of fusion was obviously important, and

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there are many other features that might well be important.
Q Did he discuss with you what features there were and what would be important and what would have to be done and what the problem was? That's a multiple question but I'm trying give you --
A Look, we had a very broad discussion at the very beginning of the uncertainties of the work, all the things that could go wrong. And, indeed, the other issue is demonstrating it because there's a question of experimental design, of choosing an experiment that would enable one to prove that the antibody was expressed on the surface and phage and would fold in a functional form.
Q Was there an experiment that he identified as one that he wanted to follow in terms of steps? "He" being Chiswell, and the time being prior to the beginning of McCafferty's work?
A I simply can't remember.
Q And can you remember his list or litany of possible problems at that time?
A At that time I think I was the main person concerned with possible problems because

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I wanted to anticipate the problems and make sure we made the right design and experiment.
Q And did you help design that experiment?
A Yes, I did.
Q What were the points, the problem points, that you designed around, so to speak, or designed to meet?
A Well, I think one of my concerns was that, if we went back to the early Smith work, that what this had shown was expression of a peptide on the surface of phage, and it had involved a selection of those phages with polyclonal antibodies. And it was well-known that, first of all, peptides had a disordered structure and that, furthermore, polyclonal antibodies would often recognise linear forms of peptide.
So that my concern was, if we expressed the antibody on the surface of phage, how would we know that it's folded? If, for example, we had anti-serum against the displayed antibody, and repeated the experiment in the same way as Smith, how would we then know that the antibody was folded on the surface of phage? So I was very keen to use an antibody that we had worked on, which was

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the D1.3 antibody, which is directed against hen egg white lysozyme.
Now, this antibody had been solved by Roberto Poljak and his group --
Q Had been what, I'm sorry?
A Solved. In other words, the structure of the antibody had been solved crystallographically by Poljak.
And what this showed was that the antibody -- sorry, that the antibody was making contact with lysozyme across a wide surface, so we could define that the antibody had to be folded in order to be able to bind lysozyme because there were several, what are called, discontinuous epitopes. So several bits of the antibody surface, from different parts of the polypeptide chain, came together to bind to lysozyme.
So that I think the D1.3 antibody was something that I suggested we should work with because we knew, if we could demonstrate that the phage bound to lysozyme, then this would show, would be one way of showing, that the antibody was folded on the surface. And, you know, I had discussions with Chiswell about it, and McCafferty and me. They agreed.

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Q Were there any other problems that you identified at this time? And, again, this time being prior to the commencement of the experiment by Dr McCafferty?
A Well, again, the other question is: How do we show that the thing has got binding activity?
Now, in Smith's papers they had shown binding activity implicitly by selecting the phage, by demonstrating that they got enrichments of filamentous bacterio phage.
One of the points that I suggested was that we should use a direct binding assay; and that was the binding of -- essentially, to bind the phage to a lysozyme coated surface and then to come along with anti-phage anti-serum. And, in fact, the reason why that was possible was because, for some years, some years' earlier, I had got hold of a stock of, I think it was, sheep anti-phage anti-serum. At that stage we were looking at the use of that serum to purify filamentous bacterio phage for sequencing, and, rather than using polyethylene glycol, which was the standard, I was quite interested in the use of the anti-serum. So, therefore, I had the anti-serum -- actually, quite

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a lot of it -- at the back of the freezer. So, being aware of that reagent, I would say, "Well, hey, look, let's go and do it."

Now, I can't remember exactly where the idea came from, but certainly the availability of that reagent was something that makes me believe I may have suggested it. But, equally well, it may have been suggested by Chiswell or McCafferty. But certainly I had the reagent to hand for that purpose.

Q Any other problem that you felt needed to be addressed experimentally?

A Well, one of the things that was worrying me about the whole project -- well, let's say, there were obvious things like whether the antibody would compromise phage infectivity. Actually, that was something that was worrying Chiswell and McCafferty; it, you know, was a possibility.

I was much more worried by other things; that was that, if the antibody folded up, it might well be in equilibrium with an unfolded form. And so I was concerned about essentially having linear epitopes on the surface of the phage as well as the folded antibody.

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Now, that was worrying because, if you have an equilibrium -- or, indeed, even if there weren't an equilibrium; even if they'd been trapped in different confirmations; that I had some mixture of folded and some mixture of unfolded or incorrectly folded antibody -- although the ELISA, that is the use of the direct binding assay that I described to you, would identify the presence of folded antibody, which, of course, is very encouraging, when we moved to the selection stage, we were obviously worried about whether this unfolded antibody might be sticky and compromise the selective process.

So, essentially, if we had a high background of binding of the filamentous phage, this could compromise the specific binding signal and mean that the selections didn't come to anything. So I was rather concerned about that.

That was a problem that Smith hadn't faced, because he had anti-serum which was binding to, essentially, unfolded peptide. So his anti-serum should recognise the various confirmations of unfolded peptide that were present. In our case, we might have to distinguish between a folded and unfolded antibody.

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So I was concerned about the background binding of phage.

Q Any other --

A And --

Q -- concerns? Sorry.

A So, therefore, as a way of dealing with that, and, again, I can't remember exactly who suggested this, we undertook, to the best of my recollection -- we, in fact, did observe that there was non-specific binding of the phage -- I can't remember exactly whether it was due to the antibody or was just an intrinsic feature of phage -- but we introduced the use of milk powder in the binding and the selection systems.

And that was something -- which, again, I can't be sure who suggested that -- but it was something that I could very well have suggested because we'd been working with antibody single domains and we knew that we had a high background, if you just took an isolated antibody single domain and tried to do an ELISA reaction to look at the direct binding of that, and we could destroy the unspecific binding by the use of milk. So that was another experimental feature that we had to build into this project.

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Q I'm trying here to give you the opportunity to give me your full list, if you can. So I'll press you one more time. Is there something else that --

A Well, look --

Q -- you might want to say here?

A I don't think I want -- I don't want to say any more. Those were the things that seemed to me to be the most important. There may well have been other -- you know, until you do it, you simply don't know.

But they were the ones that were uppermost in my mind at the time, particularly in terms of the experimental design; how we designed the experiment in such a way that we can avoid these problems arising.

Q Now, did I understand you that, in assessing what the problems might be, you studied the Smith articles? Is that correct?

MR VEZEAU: I'm going to object to that question as lacking a foundation.

THE WITNESS: Obviously what I did was to study -- I remember looking at the first article, the Smith '85 article and, as I think I've explained earlier, there were a number of

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limitations of that, including the fact that one is cloning into inside a protein gene.

As I said earlier, I was aware at that stage it was also possible to go into the end terminus, but I don't remember at that point reading the Parmley and Smith paper. I might well have done but I simply don't remember it.

BY MR SKILTON:

Q The next sentence in context then -- again, going back to 2.1, page 0144 under history, reads as follows: "John McCafferty and myself devised a potential solution and made the key oligonucleotides during the Autumn of 1989."

Did they discuss -- either one of them discuss that sentence with you prior to McCafferty undertaking his experiment?

A All I remember was that they told me that they had -- that they had thought about it to the extent that they had come up with a design. I don't remember them telling me that they had actually done it.

Q What was the design that they told you they had come up with?

A Well, it was a cloning site at the end terminus of the antibody genes. But I can't

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remember if the vector they had used already had a cloning site there or whether they were, essentially, designing a cloning site to accommodate the antibody fragments with the cloning sites that we'd been using, which were introduced by the PCR oligos.

Q And it's that design of the cloning site that they contended was the invention, is that correct?

MR VEZEAU: I object to that as lacking a foundation.

THE WITNESS: At one point, when we were discussing the early work, as I've explained to you, they believed that the cloning site might be important. So we're talking about very -- we're talking about directly after the antibody proved to be expressed, they felt that might be a key element.

I think he's over-stating the case at this particular point. It's certainly not a view that I would agree with, that those oligonucleotides were key. I didn't agree with it then. I don't agree with it now.

BY MR SKILTON:

Q The end terminus, though, that which

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they had already selected was what was used experimentally, was it not, by McCafferty?

A They cloned it at the end terminus or within a residue or two of it.

Q And that was, essentially, work that they had brought with them to your laboratory, is that right?

MR VEZEAU: I object to that as having no foundation, and that is also indefinite.

THE WITNESS: Sorry, they had brought? Sorry, could you repeat the question? I got a bit lost there.

MR SKILTON: I'll rephrase it.

THE WITNESS: Thank you.

BY MR SKILTON:

Q When was it, to your understanding, that they had created this design?

MR VEZEAU: I object to that as being indefinite with respect to "design".

MR SKILTON: I'm using, I believe, the term the witness used in his last answer.

MR VEZEAU: Same objection.

MR SKILTON: (To the witness) You may answer, subject to the objection.

THE WITNESS: Sorry, so the question

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is? Can you repeat the question? I'm sorry.

MR SKILTON: It's all right.

THE WITNESS: The last question.

COURT REPORTER: "Q. When was it, to your understanding, that they had created this design?"

THE WITNESS: Created the design? So, in other words...

I don't know when they made the phage, if that's what you mean, with the altered site; whether they had made it before or whether they made it in the laboratory when John McCafferty started the work.

I was certainly aware that, before they came, they said that they had designed an oligonucleotide for this purpose, but I simply don't remember whether they brought with them phage in which they had made these changes.

MR SKILTON: Why don't we take our lunch break now -- it's almost 12.30 -- and commence again at 1.30, if that's appropriate for everybody?

MR VEZEAU: That's fine.

VIDEOGRAPHER: We're going off the record. The time on the video monitor is 11.24.

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(A short recess at 12.24 pm)
(Resumed at 1.33 pm)
VIDEOGRAPHER: We are back on the record. The time is 12(sic).36.
BY MR SKILTON:
Q Dr Winter, when was the first time you met John Walton, the gentleman in the room today?
A Sean Walton. I don't know.
Q Have you had more than one meeting with Mr Walton over time?
A Yes, I have.
Q What's the circumstance of the meetings or meeting? Is it in relation to patent prosecution issues?
A Yes, I think so. I think I met him once at a social function, to my surprise.
Q I'm going to focus you on the US 108 patent and the application that was filed in the United States.
Do you recall whether or not you have seen any filings by CAT in the United States patent office with reference to the 108 patent?
A I have no specific recollection of it.

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Q Did you ever talk to attorney David Clough, another person in the room today, prior to getting ready for your deposition?
A I think I met him. It may have been many months' ago or at an earlier stage. I simply can't remember for sure.
Q Might it have been in preparation for your deposition for the last case, the 793 case?
A Possibly.
Q Do you recall whether or not you ever worked with or consulted with Mr Clough with reference to the prosecution of the 108 patent in the United States patent office?
A I don't specifically recollect it.
Q Do you recall working with attorney Walton with reference to the European patent proceeding relating to the European equivalent on the 108 patent, the McCafferty patent?
A I don't recollect it. But I have to say, all of these, as far as I'm concerned, are details. I wouldn't necessarily be expected to recollect unless he'd played a major role. I can't say that I didn't necessarily get a phone call from him on that subject, or I may even have signed

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something; you know, written a letter to him at that stage. But I simply don't remember it and I don't think it was important.
Q Well, is it fair to say that, from your point of view, you didn't play -- you did not play a major role in the prosecution of the 108 patent?
A The prosecution of the 108 patent in the United States now --
Q Yes.
A -- you're referring to? I would say that the prosecution is the state at which it goes from filing. You're looking at the period of filing on to grant?
Q Right.
A Is that what you're talking about?
Q Yes.
A I would say that I didn't play a major role in the prosecution.
Q Can you describe any role that you played in the prosecution of that patent, the 108 patent, in the United States patent office?
A Well, I probably played some role but I can't actually remember what it was.
I was mainly concerned with the

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repertoire patent and discussions about that. Obviously questions in relationship to the McCafferty patent would come my way from time to time, but certainly I was less involved.
Q Do you recall any questions that did come your way from time to time with reference to the --
A No.
Q -- McCafferty patent?
A No.
Q Let me direct some questions with respect to the European patent, the version of McCafferty, and specifically after oppositions were filed in Europe.
Are you aware that, in fact, oppositions were filed in Europe?
A I think I was told oppositions were filed, yes, although whether there was one or two I don't know.
Q Did you read any of the opposition papers?
A I don't think so.
Q Do you recall whether or not you read any arguments that were made by Cambridge in response to any of the opposition papers?

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A I don't think I did. Can you tell me --
Q I'm sorry, would you repeat that?
A I said I don't think I did. Can you -- it might help to refresh my memory if you can tell me what period this was.
Q We're talking -- let's talk about both in a window of, roughly, in the US 1995, roughly, through 1999. So a 4-year period.
A I see, okay. So 1995 to 1999.
Whereas it is quite likely that I would have had discussions with Ron Jackson in Cambridge Antibody Technology before 1996, I had very little to do with Cambridge Antibody after 1996. And so, therefore, it would have had to be a specific request put to me, and I certainly have no recollection of it.
Q With respect to the opposition proceedings in the European patent office -- I'll give you the range of, roughly, 1997, January 1st 1997, through today -- have you had any role, active role or consulting role, with respect to those opposition proceedings?
A I certainly have not had an active role. It's quite possible I could have been asked

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a question or two, but I don't think so.
Q You were move actively engaged in the repertoire or library patent --
A Correct.
Q -- in the US. Who was your lawyer, US lawyer, in prosecuting that patent, if you recall?
A The US lawyer? I don't think I have a recollection of that.
Q Can you tell me who the UK lawyer was?
A I think the UK was Sean Walton.
Q Have you been advised as to the difference between European patent prosecution proceedings and US patent prosecution proceedings?
MR VEZEAU: Now you're getting -- you're specifically asking the witness as to what he was advised of.
(To the witness) And I would caution you that if, in responding to this question, you have to reveal advice you received from your counsel, you should not do so.
MR SKILTON: The question called for a yes or a no.
THE WITNESS: I never answer

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questions that call for yes or no.
BY MR SKILTON:
Q Well, I don't want to expose attorney/client information. Really I'm giving you, if you will, the privilege of answering yes or no without disclosing content. That's the way I phrased the question.
A Could you repeat the question, please?
Q Yes, I will.
A I'll try to answer it, if I can, yes.
Q Did you ever obtain an understanding -- I'll rephrase it -- as to the difference between the prosecution of a United States' patent and the prosecution of a patent in the European patent office?
A Well, I was aware there were some differences; one of which appeared to be enablement.
Q What is your understanding of that difference?
A Well, my understanding of it, and I could be wrong, was that --
MR VEZEAU: Doctor, if this

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understanding resulted from advice you received from your counsel, you should not divulge that advice.
If you have some other understanding, that's fine, but you are not to reveal the discussions you've had with your counsel.
THE WITNESS: No, I had an understanding which was nothing to do with advice from counsel.
BY MR SKILTON:
Q What was that understanding?
A The understanding was -- and I don't know how I picked that up, but at some point I picked it up -- that, in US patents, enablement was considered to be much more important than in European patents.
And I think the context in which I picked that up related to the humanising of rodent antibodies, which was another of my patents, and the requirement for a lot more elaborate arguments than one would have felt were necessary.
Q I'm going to state some questions, which I believe I know the answer to, but just to make the record clear.

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You don't believe yourself to be an expert in the United States patent law, is that correct?
A Compared to the man in the street, I am, but I don't -- I'm not a trained patent lawyer, yes.
Q Are you aware of the obligations that a patentee has with reference to disclosures relating to the invention, or prior art that's relevant to the invention, in the United States patent office?
A My understanding is that an inventor has to be open about his understanding of the prior art and disclose all the prior art he's aware of.
Q Have you ever been given any written materials which state the duties as they relate to an inventor or an applicant, by anybody at any time?
A I think on the -- I think by the mid-nineties I had noticed -- I don't know whether it was the first time I'd been given the forms, but I remember noticing there appeared to be statements on forms relating to the US patent office, that one had a duty of disclosure. But exactly when that was, when it impinged on my consciousness, I don't

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know.
Q What you do recall relates to, I'm going to say, the documents that you signed? Is that a fair characterisation of what you just said?
A I don't know. I don't think I was aware of it early on. In other words, it may well be that I signed documents, but I do not know whether at that point I was aware of that.
I have become aware of it over a period of -- probably by the mid-nineties I would say I was aware of it, but I don't know whether early on I was aware of it, even if I should have been. I can't be sure.
Q Have you seen any regulations relating to the duty of disclosure? The United States Federal Court regulations?
A If the regulations were on the patent form, then I must have seen it, if I'd signed the document, but I don't recollect receiving a, sort of, separate brochure or anything like that on it, or studying it particularly.
Q Have you ever read a document which can be described as a George Smith grant application or George Smith grant?
A Have I ever --

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Q Ever?
A -- read a document that could be described as?
MR VEZEAU: Yes, a good point. I'm going to object to that question as rather indefinite and vague.
THE WITNESS: A document that could be represented as a grant application? Was that it?
BY MR SKILTON:
Q I will state the question to be -- I'll restate it.
A Thank you.
Q Have you read the Smith grant application?
A No.
MR VEZEAU: I'll object. Just a minute. Well, we got a "no" there, but I'd object to that question anyway for lack of foundation as to characterisation of that document.
MR SKILTON: Well, that's why I phrased the question as I did in the first instance. So I'll try again. Let's just --
THE WITNESS: Can I just characterise my --

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MR SKILTON: Please.
THE WITNESS: Can I just say something about my answer?
MR SKILTON: Please.
THE WITNESS: I understood that, from reading the testimony of the other witnesses, that there was a grant application that you were going on about, which was in the late eighties.
I was not aware of that grant application. In fact, I don't think I've ever seen a grant application by George Smith.
BY MR SKILTON:
Q Up and to this day?
A Yes, I think that's correct.
I may have seen a paper more recently. In fact, I think I did see a paper more recently, within the last few years, but certainly not at the time we're talking about.
Q What is the paper you're alluding to here?
A I don't remember. There was a paper I was asked to review, make some comments on for a journal. This paper must have been in the last three or four years.
MR SKILTON: Mark that, please.

(Exhibit 10 marked for identification)
BY MR SKILTON:
Q Dr Winter, I place in front of you what has been marked in these proceedings as exhibit 10, which I will describe as a document bearing the Bates stamp numbers of CM040479 through CM040554 inclusive, I believe.
MR VEZEAU: I can tell you, it isn't.
MR SKILTON: It is not?
MR VEZEAU: Maybe it is. I can't tell you anything.
MR SKILTON: Thank you.
I will represent that this is a document that your office has identified to us as the one that was in the files of Sean Walton.
MR VEZEAU: My office did not represent that this was a document. Let's make that clear.
MR SKILTON: All right. Well, again, these pages, to my understanding, were in the files of Sean Walton. Do you agree with that statement?
MR VEZEAU: No. I have no idea about that. If you say that's what you were told

by my office, that's fine. I'm just not sure I am aware of that. (Document handed) Thank you.
(Exhibit 11 marked for identification)
MR SKILTON: The witness doesn't have to look at this.
(To the witness) This is the lawyers talking just to be sure we have the right document on the record.
THE WITNESS: Okay.
MR SKILTON: I have put in the record exhibit 11 which, on its face, purports to be a letter from one Ms Stueber to Mr Harth dated April 25. I'll read the sentence in, and let's see whether the documents that I've identified are consistent with this letter.
"A copy of the Smith Grant application from Mr Walton's files is numbered CM040479 through 40554."
I believe those are the numbers I used in my identification.
MR VEZEAU: Yes, but we have also learned that this is not a document, during the course of discovery, and it's a deposition which your side has taken, so I just alert you to that. That's why I objected to the characterisation of

this material as "a document". In other words, John, there are several computations here.
MR SKILTON: I rephrased it, you may recall, as "pages" taken from.
MR VEZEAU: Oh, "pages" are fine. I have no problem. I mean, I appreciate you putting this letter in front of me. I can tell you, this is the first time I've seen this letter, but I have no problem with it.
Now, did we mark this as an exhibit?
MR SKILTON: We did.
MR VEZEAU: Okay.
BY MR SKILTON:
Q With all that folderol, have you ever seen these pages inclusive, or any of them, prior to this very moment?
A Well, I've looked at the front, and I've looked at a few pages inside, and I can say I've not seen the ones I've seen.
If you want me to say whether I have seen any of the pages, I'm going to have to go through them one-by-one and search my memory banks, but I'd be surprised if I'd seen them.
Q At least as you sit here today, and

based on what you have looked at, it's not a document or set of pages that you're familiar with?
A No.
Q Is that correct?
A Correct.
Q With respect to the various communications you had with Sean Walton, do you have any recall of any conversation concerning a Smith grant or a Smith grant application?
A I believe that at some point I was asked, either by Sean or it might have been Sean through Ron Jackson, had I --
MR VEZEAU: At this point, those communications are privileged. I think you need to listen to this question carefully and, if you can, answer it yes or no.
If you can't, we'll have to deal with it, but please listen to the question carefully because the actual conversations may be privilege.
THE WITNESS: I see.
MR VEZEAU: That's my concern.
THE WITNESS: Okay.
MR VEZEAU: Thelma, do you think you

could read the question back, please?
COURT REPORTER: "Q. With respect to the various communications you had with Sean Walton, do you have any recall of any conversation concerning a Smith grant or a Smith grant application?"
THE WITNESS: I have no specific recall of a conversation with Sean Walton on the subject.
BY MR SKILTON:
Q Were you ever asked to review any arguments made in the European patent office by CAT concerning the Smith grant or Smith grant application?
A No, I was not.
Q Do you have any general recollection of any discussions concerning the Smith grant or Smith grant application with Mr Walton?
MR VEZEAU: I believe that question has been asked and answered and I object on that basis.
MR SKILTON: I will say that his answer, as we understood it, was "specific recollection".
MR VEZEAU: Well, I'm not sure what

the difference is between "specific" and "general", so I --
MR SKILTON: There may be none.
MR VEZEAU: I understand. That's why I maintain my objection.
MR SKILTON: I'll ask it again.
THE WITNESS: Yes, thank you.
BY MR SKILTON:
Q Do you have any general recollection of any conversation between yourself and Mr Walton concerning the Smith grant or Smith grant application?
A I have no general recollection of a conversation with Sean Walton, but I was aware that the matter had been raised.
Q How did you become aware of it?
A Well, that's what I don't know. I believe I may have been asked a question by Sean Walton or by Ron Jackson, and I simply don't remember now who would have raised it.
Q Do you remember what the question was?
A The question was something like, "Have you ever seen a grant application from George Smith?" And my answer was the same as I've

given you, "no". And I don't think I was given any more detail than that.
Q Did you ever have any conversation with David Clough concerning the Smith grant or Smith grant application?
A I don't think so.
Q Does the name Jim Wells mean anything to you?
A Yes, it does.
Q When was the first time you met Mr Wells -- Dr Wells?
A I don't know for sure, but I think it was in the mid to early eighties.
Q Do you recall the circumstance of that?
A I don't recall the specific circumstance of when I first met him. I have a recollection of having met him in the United States.
Q What was discussed, if anything?
A One of my PhD students, Paul Carter, had gone to work as a post-doc with Jim Wells, and I think we were just discussing protein engineering in general.
MR VEZEAU: John, did you want to

take a minute?
MR SKILTON: If I may. I'm sorry.
MR VEZEAU: Yes, we'll just take a minute or two.
VIDEOGRAPHER: We're going off the record. The time is 1300(sic).
(A short recess at 2.00 pm)
(Resumed at 2.09 pm)
VIDEOGRAPHER: We're on the record. The time is 1309(sic).
BY MR SKILTON:
Q Dr Winter, before we took the break we were talking about Jim Wells.
A You were.
Q Right. At any time did you become familiar with the work that Jim Wells was doing?
A Which work?
Q Well, let's talk about on the human growth hormone?
A I became familiar at some point that Jim Wells was working on human growth hormone, yes. I can't remember exactly when it was.
Q Are you aware that he ultimately published a paper with Dr Bass concerning the display of human growth hormone on phage, bacterio

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phage?
A Yes.
Q Did you read that paper?
A Yes, I did.
Q Did you read it at the time it came out, it was published?
A Yes.
Q Did that give you information about work that Doctors Bass and Wells had been doing that you didn't know?
A Yes.
Q Prior to reading that paper had you been aware, however, that they were seeking to find a route to display on bacterio phage the human growth hormone?
A I was aware that they were trying to do that, that's correct.
Q When did you become aware of that fact, if you recall?
A I became aware of the fact that they were trying to display human growth hormone in -- it must have been some time in the spring of 1990.
Q What was the circumstance of that? How did you become aware of it?
A I'll need to take a step back,

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actually, to explain. It will probably save you a lot of questions and save your voice.
Q Thank you.
A In 1989, in the summer of '89, Andrew Griffiths and I were having a discussion in the laboratory about expression of or display of antibody fragments on filamentous phage.
At that point Cara Marks, who was the wife of Jim Marks -- she was a PhD student at my laboratory, and she had come from Genentech -- and Cara said something like...
We were discussing this. In fact, I was discussing it with Andrew Griffiths, and we had actually thought that we would go ahead with the use of phage; we were equivocating between lamB and filamentous phage at that point.
And she said -- she was on an island behind the main bench -- she'd been going in and out -- and she suddenly popped up and said, "Jim Wells has been trying that for a while." That was actually complete news to me at that point.
So I said, well, you know, "How long has he been doing it for?" She didn't say exactly what he'd been doing, but she said, well, he'd been

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at it some time before she left Genentech.
So at that point I said, well, you know, "Does it work?" She said she didn't know. But she said, "You should contact Jim."
Now, as it so happened, there was a meeting, a protein engineering meeting, on the island of Spetses in Greece. I was going to that, and, in fact, Cara was going to it, as was Jim Wells. And I decided that the best thing to do would be to bring the matter up then rather than ring him cold. I knew Wells but not incredibly well; he's a very friendly person, but I felt that this was something that I wanted to talk to him about face-to-face.
So, in fact, when I went to Spetses, Wells wasn't there at the beginning; he turned up I think a day or two late. I think he came in actually during a performance, some kind of Greek tragedy, which was extremely boring, and we were all forced to watch this. And it was dark. And I remember Jim came in. I didn't see him at first but Cara saw him, and she came to me afterwards -- I think later that evening or possibly the following morning, I can't remember -- and said, "Jim is furious with me for telling you about

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Genentech proprietary work." And I said, "Well, I'll talk to Jim."
Jim Wells was very circumspect. It was like a different person. He was pleased to see me, but said that Cara never should have said anything about their work. And, actually, I agreed with that because she placed me in quite a difficult position; we had delayed working on the phage display and I think it put Jim in a very difficult position because he wasn't, obviously, exactly sure what Cara had told us -- or told "us"; myself and Andrew Griffiths.
I explained to him that all I knew was that he'd been trying it. And he said, "Well, there's lots of problems." And I said, "Well, look..." You know, I knew Jim Wells was an excellent scientist.
I pointed out that we were involved in this scientific competition with Richard Lerner, and we would like to express antibodies, and would it be possible to collaborate with him since it appeared to be so difficult. And he said, "Well, it could be a problem because of Genentech corporate policy, but please could I refrain from discussing the idea with anybody?" And I said,

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"Yes, you can take this as being confidential."

This was actually a discussion I had completely in private with Jim. He'd been very anxious to make sure, I think on that occasion, there was only myself and himself. Cara, I don't believe, was there.

So I said, yes, I could say I can keep this confidential; it's to neither of our advantages to discuss this, particularly if it works, and he said he'd look into the possibility of a collaboration, but he said it's really too early to say; for commercial reasons, and kind of others, he couldn't really say anything about it at this point, apart from the fact that it's not straightforward.

Now, I have to say at that point I was rather disappointed. I was disappointed actually to hear that there were considerable difficulties. I'd been alarmed to hear from Cara that he'd been working on it for a period of time, and he appeared to be saying it doesn't work or that there's difficulties, but he didn't actually say what the difficulties were.

So, after that Spetses meeting, we went home.

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And I had at least one conversation on the phone with him before Christmas -- I can't remember exactly when it was --

Q Of nineteen --

A Christmas '89 -- whether they would be willing to collaborate with us for expression of antibodies. And he kept saying, "Well, actually, it is difficult." He seemed to be emphasising less the commercial issue than it was difficult, but obviously he wanted it to be kept a secret. And I said, "We can live with that because it's not in our interests to disclose it to the Lerner camp."

When I talked to David Chiswell, David Chiswell -- sorry, when David Chiswell...

I didn't say anything to anybody about those discussions until such a point when David Chiswell mentioned to me what he obviously regarded as his idea of using filamentous bacterio phage. And I have to say that I had actually told Jim Wells that we would not pursue -- pending the answer from him, that I wouldn't pursue it.

But, by Christmas, I was becoming uneasy that I was being strung along; I didn't know what the difficulties were, it didn't seem to get any more positive. Actually, I have no idea

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exactly what was going on in Genentech at that time.

I felt, actually, that I'd been compromised; I'd been compromised by Cara telling me that Jim was doing it, and I'd found myself compromised by the fact that I had, in Spetses, agreed for the time being not to undertake work on filamentous display of antibodies.

But I was still hopeful that we could come to a deal whereby we might get some information that would help us to express antibodies and to be able to select them.

Chiswell argued that, to the extent that he'd come up with the idea independently, and also to the extent that we had agreed to keep our idea secret, that I should not disclose the proposed experiments, namely, our own attempts to display antibodies on phage. I agreed to that, but I felt very uncomfortable about it.

Later in January -- no, it wasn't January. I don't know when it was exactly. It was either January, February or March of 1990 I went to a meeting in the States, and that was the meeting I mentioned to you where Jim Wells was. In fact, I was only there very briefly; something like

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a flying visit of a day or two. I think I might have stayed overnight.

The meeting was one in which Jim Wells was featured to appear, and he came. In fact, because the meeting was fairly intense, I didn't get a chance to talk to him at the meeting itself about the phage, the possibility of phage display, but he offered to give me a lift to the airport. And I asked him whether they were still interested in collaborating. And he said, "Well, you know, it was extremely tricky. They weren't in a position that they could collaborate at this point."

And I believe I said at that point, "Well, you know, you keep going on about difficulties, but does it work?" And he said something like, "Put it this way, we're expressing a protein, and it's recognised by antibodies, but we've also got a mutant, and we know that the protein doesn't bind at all well to antibodies. But, when we look at binding on the phage, when we look at binding of the phage, we don't see any difference."

And then, in fact, he said, "Look, I shouldn't have told you anything." Really, you

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know. And I said, "Well, look, you know, we're going to have to go our own way."

And I think he then told me that Paul Carter was contemplating displaying antibodies on phage in Genentech. But he didn't say he would or he wouldn't; he just said "Paul Carter is quite interested if we get it to work", whatever the thing was he was expressing.

Now, at some point, I think he might have said he was working on growth hormone, but that would be surmise on my part. What was represented was that there was a polypeptide that was expressed and it was recognised by antibodies, but there was a problem in that two proteins, which he said were known to have very different binding characteristics, but the same on phage.

Later, when I read Wells' paper, I realised that what he must have been going on about was the avidity effect, because what he did was to make a way of reducing the valency of display of growth hormone on the surface of phage.

But actually that wasn't the interpretation I placed on it at the time. I didn't have the faintest idea exactly what was happening.

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It seemed to me that what he was talking about wasn't so very different from Smith; he was saying that his protein, whatever it was, bound to antibodies and, furthermore, he didn't see any difference between the binding of the wild type and the mutant protein, which actually suggested to me that they were unfolded, or that there was some stickiness, and I, in fact, wondered whether he must mean that.

But just as we were more or less at that point, the airport loomed and, basically, I had to get off. But he wasn't willing to say any more. He said, "I've told you more than I should say and, please, we must keep this confidential." And I agreed to do that.

Q When Cara Marks first raised the issue in the lab, can you tell me approximately what the date of that was?

MR VEZEAU: Raised the issue?

MR SKILTON: The issue--

MR VEZEAU: Made a comment?

MR SKILTON: -- of the work of Jim Wells.

MR VEZEAU: I'm not sure she -- I'll object to that as lacking a proper foundation.

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BY MR SKILTON:

Q Is that a fair reference?

A Well, the conversation that I've told you about, that first conversation, which was actually I think, apart from Spetses, the only other conversation I had involving Cara Marks in respect of phage display, was probably around April or May. It was basically of 1990. Is that right? Sorry, '89, 1989. So we had had...

It was really the discussion that Griff and I were having, Andrew Griffiths and I were having, kind of, in the lab, in my own laboratory, where she'd, sort of, unexpectedly intervened.

Q She overheard it apparently?

A She overheard it, yes. She wasn't -- I wasn't discussing it with her. She was working on something completely different. I've forgotten what it was now; it was probably catalytic antibody.

Q You were talking, were you, about issues of display of antibodies with Dr Griffiths?

A Correct; whether now, having shown that we could make an antibody library, we could look at the different systems that might be

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available for selecting it. And we therefore looked at -- we were discussing, I think, the Brian Seed work with eucaryotic cells, we were discussing lamB, and we were discussing phage display, and probably we were turning to phage display. But, at that point, Cara, sort of, said to our great surprise, like a bolt from the blue really, that Jim Wells has been trying that for a while.

Q And what -- --

A Sorry, had been trying display of proteins. I had no reason to believe he was trying antibodies at all. In fact, obviously he wasn't, it turns out.

Q Did she say anything about the issue of long chain, aminoacid chain proteins in this first conversation?

A It was more that this was something that I was discussing with Andrew Griffiths, and really pointing out that the only work that we'd seen from Smith seemed to relate to the selection of unfolded proteins with anti-serum that we believed would react with unfolded proteins.

Q I'm informed that Dr Marks unfortunately passed away, so I'm going to do the best I can to exhaust your recollection on an issue

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that she can't comment on.
Do you recall the issue of taking phage display passed the peptides or short aminoacid chain proteins into a longer chain was what she was commenting on in terms of her interruption of you?
A I'm not sure exactly what she was commenting on. The particular issue that Andrew Griffiths and I were discussing at that point was whether, if you had even a long polypeptide chain, whether it would be folded and would fold on the surface of phage, and whether there would be the kind of problems I've alluded to before. So we were having a discussion, a fairly wide-ranging discussion, on those points.
Q She wasn't invited? She was, if you will, someone who overheard it? Is that a correct description?
A Yes. She was a member of my group, and I had no reason particularly to try to be secretive. But, in fact, she was going in and out of the lab doing an experiment at the time, and she just, sort of, came in, obviously heard part of the conversation, and said something like, "Hey, you guys, Jim Wells has been trying to display proteins

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on phage for some time."
Q My question was specific. Did she make any comment in terms of the length of the proteins in terms of the aminoacid chain? Did she say anything that it's a long chain or?
A I don't think she did.
Q Did her comments --
A She wasn't really. I mean, it was more exactly as I've said. She blurted this out, then she realised she probably shouldn't have said it because she was bound under confidentiality at Genentech. I actually wish she hadn't said it, because it now made me feel quite uneasy about how to proceed. But, equally well, I was disturbed that it appeared to be the case, if Jim Wells had been trying it for some time, that either he was going to get there imminently or, in fact, it was incredibly difficult, and either of them I didn't like the implications of that.
Q You were either going to be in a race or you were going to be involved in a wasted effort?
A Yes, and at that stage I simply didn't know what it was. It sounded to me more like that it was going to be a wasted effort,

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because I knew he was very competent and, if it was straightforward, he could have done it very quickly.
Q Did her comments at all influence you to go toward the phage, the bacterio phage vector?
A If anything, her comments encouraged us in the other direction, and so the work that Andrew Griffiths started was work on the lamB expression, and what I decided to do was to put a hold on any phage attempts until we'd talked to Jim.
Q You had mentioned that hold. Are you saying that, as of the time of April or May of 1989, you had decided to go ahead with a phage vector experiment?
A No, I'm saying that we decided to put it on hold. We decided to go ahead with a lamB expression, which was the bacterial expression.
Q And that's experimental work, I take it, that Dr Griffiths did?
A Yes.
Q Did that work commence after this conversation with Cara Marks?
A I think it did.

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Q You, therefore, decided to, what? Find out a little bit more about what had happened in the Wells' laboratory?
A I decided that, if it was possible, we'd enter a full-blooded collaboration with Jim Wells because, what I'd heard that moment, which was only fleeting from Cara, was that he'd been at it for a while, and therefore I thought he'll be well ahead of us on it, and therefore maybe it's possible to do something with him to get advantage of what he's already done.
Q When was this meeting in -- help me spell the word. Is it Spetsa?
A Spetses, actually, I think. It's S-p-e-t-e-s(sic).
Q Is that one of the islands?
A It is.
Q When was this meeting?
A It was at the end of August 1989 or early September.
Q You did describe your contact with Dr Wells at that time.
A Yes.
Q Did his comments at that time cause you to continue to put your project on hold?

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A I think it did. I think we decided to hold off until such point, as it wasn't too long, he could confirm that we could go ahead and collaborate. I was optimistic at that point that we could collaborate.

Q Now bring, then, Dr Chiswell into this chronology. Was it before or after your meeting with Jim Wells that you had your first conversation with Dr Chiswell concerning the bacterio phage vector?

A I believe my conversation with Chiswell was towards the end of the year, of '89, so that I think it was quite -- it was directly before McCafferty came into the lab, or it might have been January 1990.

Q So, at that time when you talked to Dr Chiswell, your belief was that Genentech, through Dr Wells, was in the process of attempting to develop the bacterio phage vector for purposes of displaying proteins. Is that a correct statement?

MR VEZEAU: Objection. Lack of foundation.

THE WITNESS: What I was aware is that Genentech had a project, which they didn't

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divulge at that point, which involved trying to express proteins on the surface of phage, but I didn't know whether that project, at that time, was directed towards just expressing long polypeptides, namely, not necessarily folded, or whether it was folded.

I assumed it was folded but actually, now I think about it, I didn't know, because Cara Marks was very circumspect and Jim Wells was as well, so he didn't really spell out what he was trying to do.

BY MR SKILTON:

Q Did you tell Dr Chiswell about your contacts with Cara and, ultimately, Dr Wells?

A I spoke to Dr Chiswell. When Dr Chiswell said, "What about trying filamentous bacterio phage?", I said, "Well, I think it's a good idea. In fact, you should be aware of this; that there has been contacts between myself and Jim Wells who, I understand, may be trying to express polypeptides on phage." And I explained the story to him, on the grounds that it was obviously strictly confidential between the two of us.

Q Did you say that you wanted to wait

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further to find out whether there might be a collaborative opportunity at that time?

A Well, I wouldn't say I was happy about waiting. I was getting very frustrated with waiting. But we basically decided that we should start.

Q Without any further contact of Wells or of Genentech, correct?

A Well, I believe that before -- before the end of -- I think I spoke to him relatively shortly before Christmas in '89, and I again reiterated my desire for a collaboration with them. And, again, he was very pleasant, but he still kept referring to various difficulties.

Q Okay. This was another conversation that would have, to the best of your belief, have preceded your first conversation with Dr Chiswell?

A Preceded my first conversation? No, I believe that, after my conversation with Dr Chiswell, without revealing my discussions with Dr Chiswell, I wanted to see, before we started that project, if Wells was willing to help us, you know, form a collaboration.

Q And that's the December call?

A That was the December call.

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Q You have a recollection of that date, am I right, shortly before Christmas?

A Yes.

Q Is there something that rings a bell in that regard?

A Not particularly, but for some reason I think it was around then. I think that it was -- you know, I think it was some time in December.

Q And is it correct that, as a result of this call, after talking to Chiswell, you concluded that there wasn't a realistic opportunity to have a cooperation with Genentech or Dr Wells and it was time, therefore, to proceed on your own?

A No, I didn't. I continued to be hopeful. I expected it to be a very difficult business and that it was going to take us some time to do the work. I didn't know what problems we might encounter. I could think of one; the kind of points I've made to you. So, essentially, when I went to see...

The point at which I decided that we would absolutely go it alone was after the meeting in the United States, when we had got essentially indication from him that there were all these

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difficulties and implicitly... He seemed to sound more negative about the possibility of a collaboration. In fact, even referring, as I said, to Paul Carter, who had been my graduate student, being interested in applying this to antibodies.

Q My notes may be imprecise, but, as I recall your testimony, you indicated that that meeting was later in 1990. Can you be more specific as to that particular meeting?

A It was some time in the spring, and I think that the...

I don't know exactly when it occurred, but I believe that, by the time I went there, we had got some kind of evidence that the antibody, that the antibody D1.3 was being expressed, but I can't recollect exactly what it was. It might have been that we had a fusion band on the gel. I simply can't remember. But I was essentially full of foreboding about the project in the light of the kind of comments Wells had made.

Q That is to say that you felt Wells was suggesting to you that it wasn't going to work, even if you had some preliminary success? Is that a correct characterisation?

A I don't know whether...

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The preliminary success was not actually something I could call success. I have to say I can't remember exactly what had been done, but when I went to the States to see Wells I was feeling slightly optimistic. I don't know why I was feeling slightly optimistic. It wasn't just that there was a possibility of coming to an agreement with Wells, because I didn't really have that.

I think it must have been based on some observation made by John; that perhaps we had got the antibody gene cloned and perhaps there was some evidence of expression. But I really can't be sure what it was at this stage.

Q Now, I understand that you're doing your best to recount your recollection of conversations that occurred, a fair amount of time ago, so I don't want to sound critical. But, rather, I want to test how precise we can be.

A Yes.

Q When you saw Dr Wells in the spring of 1990 in the United States --

A Yes.

Q -- he was circumspect? Is that a correct word?

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A He continued to be very circumspect, yes.

Q Did not disclose to you, for example, what protein it was that he was working with?

A I can't be sure of that. I think that, in the car, he used the word "protein" and I used "growth hormone", because I think I had reason to believe he might have been working on growth hormone, and he didn't dissent from it, but was very circumspect.

Q And he led you to believe -- if my notes are correct; and here just tell me where it's wrong -- that he was having trouble with his experiment, is that correct?

A Yes, he did.

Q What specifically did he say, if you recall?

A Well, he was very mysterious about what the trouble was, and it was only in the car that he, just as I was getting out, that he said that the problem was that they appeared to have displayed polypeptide, for which they had a mutant polypeptide, which they knew, in a free state, interacted; one interacted with high affinity

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against antibodies, and that the other one, that the mutant also -- sorry, that the mutant, which was being displayed on page, seemed to react, in a phage form, as though it had got a low affinity. Sorry, as though it had got the same affinity as the wild type.

Q Did that comment at all be affect your view of whether your experiment would ultimately succeed?

A As I said to you earlier, it was actually like a -- I couldn't interpret. I couldn't understand what he really meant. And the interpretation I placed on it at that stage was that he must be using anti-serum, and that this was reacting in a similar way to the native and to the mutant protein when it was on the phage; indicating that it may well be denatured on the phage, or at least that there was a major component that was denatured on the phage.

MR SKILTON: Why don't we take a break at the break point on the tape?

VIDEOGRAPHER: Thank you. This marks the end of videotape number 2 in the deposition of Dr Greg Winter. The time is 1348(sic) and we're going off the record.

(A short recess at 2.48 pm)
(Resumed at 3.10 pm)
VIDEOGRAPHER: This marks videotape number 3 in the deposition of Dr Greg Winter. We're on the record. The time is 1410(sic).
BY MR SKILTON:
Q Dr Winter, let me take you up to where my notes were as we were reviewing them.
We have you having met with Dr Wells and taken a cab back to the airport with him some time in the spring of 1990.
Is there any document that you could find or help us to find that would peg that date with precision?
A I don't think it was a cab. I think he was driving.
Q Thank you.
A I don't have a document. I've actually wondered about exactly when that date was, but I don't have anything that at the moment I can bring to mind.
There was a meeting at the Waksman Institute. That's W-a-k-s-m-a-n. And I can't remember who organised it or why I went there in the first place.

Q What City, please?
A I think it's -- it's somewhere in New Jersey.
Q Is that where the institute is located?
A Where the Waksman Institute -- I believe so, yes.
Q Now, the next time you heard anything about what Jim Wells or his laboratory was doing was when?
A It was when the paper came out. When his paper came out.
MR SKILTON: Mark that, please.
(Exhibit 12 marked for identification)
BY MR SKILTON:
Q Dr Winter, I put in front of you a document which bears the number exhibit 12 and the Bates stamp numbers MO20015 through 20. Do you recognise this document?
A It looks like the paper that Jim Wells published relating to display of growth hormone on phage.
Q This is the paper you earlier testified that you had read when it came out, correct?

A Yes.
Q The first disclosure of priority documents that underlied the 108 patent was on July 10 of 1990, is that your recollection?
A I think it was something like that. But it wasn't a disclosure; it was secret. You're probably using "disclosure" in a very technical sense.
Q Yes.
A Okay.
Q Why don't you state your understanding of what it was that happened on July 10?
A I believe that a version of the work that we were in the process of writing, or drafting, for the paper which subsequently appeared in the Nature article was filed as a priority document in the UK.
Q Was your work completed at the time that this document was filed?
MR VEZEAU: "This document" being?
MR SKILTON: The document that was filed on July 10, 1990 as a priority document in the UK.
MR VEZEAU: What work are you

referring to?
BY MR SKILTON:
Q The work you were doing with respect to phage display?
MR VEZEAU: There was a lot. John, I have to ask you to be a little more specific. There was a lot of work.
BY MR SKILTON:
Q What was the status of the work as of the filing, to your understanding, Dr Winter, as the head of the lab?
MR VEZEAU: And apparently this question refers to anything to do with phage display.
MR SKILTON: With respect to the work of Dr McCafferty, which ultimately resulted in the patent, the 108 patent.
BY MR SKILTON:
Q What was the status of that work as of that filing?
A Sorry, you apply the work of McCafferty. It was the work of McCafferty that led to that 108 patent. But, as you know, there were a variety of other inventors --
Q Thank you.

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A -- related to repertoires. So I'm assuming you're talking about: Had we finished the work described in the subsequent McCafferty paper by this date?

Q Yes, that --

A Actually, I simply don't remember.

I believe that that first priority document, that was drafted by David Chiswell and John McCafferty. I believe that I would have seen it at some point, but it was intended as an initial filing in order to obtain an early priority date at such time as we were convinced that the method worked for display of antibodies.

Q So is it fair to characterise it as a protective filing?

MR VEZEAU: That, to me, is an indefinite term and I think argumentative. I'm not sure what you mean. I object on those bases.

MR SKILTON: I'll rephrase it.

BY MR SKILTON:

Q Was this intended to protect the patent physician, to your understanding, of MRC and CAT as of the date of July 10 1990?

A It was a patent filing, and obviously a patent filing is a commercial document,

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which, in the end, could protect a commercial investment.

I'm not quite clear what you're driving it. It's a filing. What else is a filing other than something to protect a commercial position?

Q Was there concern as of that date, July 10, 1990, that Genentech, and specifically the work of Jim Wells at Genentech, was going to be the subject of a patent application?

A We had no knowledge of what Wells was doing at that time.

Q Was there concern?

A You know, that may well have been there; that Genentech might file something. We had no reason to believe they would file anything on antibodies.

We were actually much more concerned about the selection of antibody repertoires in our scientific competition with Richard Lerner. I think that was the aspect that we were concerned with. We thought if anyone was more likely...

I certainly saw the bigger threat as coming from Richard Lerner than from Genentech.

Q Did you see any threat coming from

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Genentech at that time, July 10, 1990?

A There was no specific threat, other than I was aware that they had been working on the field of display of polypeptide and had had difficulties, and obviously something could have emerged from them, but there was no specific threat that we were aware of.

Q Are you aware of whether or not there is an interference now pending in the European patent office or in the United States patent office concerning the Bass and Wells patent?

A I was certainly not aware that the European patent office allows for inference.

Q I misphrased it. Let's take it to the US patent office. Are you aware of any interference pending in the United States?

A Between?

Q Between Genentech and its Bass and Wells patent and Cambridge Antibody?

A I was not aware of that, no.

Q Are you aware of any opposition proceeding in Europe involving Genentech in which Cambridge Antibody has participated?

A I was not aware of it, no.

Q Have you ever submitted an affidavit

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stating the facts, essentially as you've stated them on this record, concerning your involvement and/or contacts with James Wells?

MR VEZEAU: May I hear that question back, Thelma?

COURT REPORTER: Certainly.

"Q. Have you ever submitted an affidavit stating the facts, essentially as you've stated them on this record, concerning your involvement and/or contacts with James Wells?"

THE WITNESS: I have not.

BY MR SKILTON:

Q Is it correct to say that, to your knowledge, the first time you've ever stated the narrative that you gave us in answer to my question was on this record on this day?

A The first time I've ever stated the narrative? I think I told you earlier that I'd had a discussion about some aspects of that with Chiswell in late '89.

Q All right, let me be a little more specific. Have you made any similar statements concerning this subject-matter under oath at any time?

A No.

Q Have you submitted a report to anybody or a chronology to anybody that contains essentially the facts that you've recited today?
A I don't think I have.
Q Now, in some of your answers, with respect to your understanding of the work of the Bass lab, you've used the word "polypeptide"?
A Yes.
Q At one point, was it your understanding that he was working with polypeptides?
MR VEZEAU: I'm going to object to the word "understanding".
THE WITNESS: I believed he was expressing, or trying to express, something that was larger than the smaller polypeptides that had been described by the original Smith paper.
BY MR SKILTON:
Q Is the human growth hormone considered a polypeptide?
A It is.
Q Do you distinguish a polypeptide, for example, from a protein generically, or is it just a form of protein?
A A polypeptide, I think most people,

and certainly I would, view in this context as being a longer form of -- well, sorry.
It is a sequence which doesn't necessarily have any defined confirmation. So, in other words, it could be a random coil or it could be a folded protein. One simply doesn't know.
Q It could be either?
A It could be either, yes.
Q And at some point in time in the chronology you became aware, in fact, that the polypeptide that Dr Wells was working on was, in fact, human growth hormone?
MR VEZEAU: Some time in the court chronology?
MR SKILTON: The chronology he gave with reference to his knowledge of the work of the Wells' lab.
THE WITNESS: So you're not referring to the paper when the paper emerged?
MR SKILTON: No, I'm not.
THE WITNESS: Okay. So at some point I may have done. I can't be sure whether I'm reading things back into it. I don't know whether he told me or whether I surmised it. I don't think he actually said growth hormone, but I could be

wrong.
BY MR SKILTON:
Q Could it have been as early as your first conversation with Cara Marks?
A No.
Q You're sure of that?
A I'm absolutely sure.
Q Did she use the word "polypeptide"?
A I don't think she was specific. I think she was listening to the conversation that we heard, and she said, "Jim Wells has been trying to do something like that."
Q "Something like that." Like what? What was the reference point?
A At that stage we were talking about expression of antibody -- the possible expression of antibody fragments on phage, and we were discussing several aspects. Obviously one of them they're rather long, and the other aspect is that they're folded.
MR SKILTON: Next exhibit.
(Exhibit 13 marked for identification)
BY MR SKILTON:
Q I'm placing in front of you what has been marked as exhibit 13, which purports to be

a memorandum dated August 21, 1990, and bears the Bates stamp number CM040137.
Is this a document that you read in preparation for your deposition?
A Yes, I did.
Q Let's look at the first sentence that mentions you by name and see whether you can add your knowledge, if any, to the contents of this sentence: "I have however been able to relay the outcome of our discussion to Aaron, Greg and Cesar."
What discussion, if you recall, was being referenced here?
A Trying to do my best to reconstruct events after all this time, I think that Chiswell produced some kind of -- or had made representations which -- probably similar to those in the position paper that you showed me earlier; I've forgotten which document that is. And, if you remember, there was a statement in there in which he had said that he had, I believe, discussed it with Sir Aaron and me, and that basically we agreed with his analysis. And I think that what this is actually saying is that there's a gap of understanding as to exactly what the position is.

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As I explained to you earlier, we were trying to identify what, at that point, seemed to be the crux of the invention, and there was a view that I had -- which, in fact, is the view that I continue to hold now -- which is that the invention was the idea -- was the demonstration of the fact that we could express folded antibodies on the surface of phage, and indeed one could select those phages using something that recognises that folded structure.

The view of David Chiswell was that there might be a significant aspect of the invention which involved the exact insertion point, and I think that his argument had been, to the extent that he and John McCafferty had identified that key insertion point, then that the invention really resulted from the work of himself and John McCafferty.

In fact, it was my view that indeed, if the invention was represented as such, then, indeed, it was the case that they would have been the key inventors on that understanding of the patent.

That doesn't mean to say that I would not have been an inventor, but I certainly

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felt that if, indeed, it had been correct that the phage was -- sorry, that the insertion point on the phage was central to the invention, then they would have had a stronger commercial claim to the intellectual property, or the specific division of spoils on the intellectual property, which, as I mentioned earlier, was a joint collaboration between MRC and Cambridge Antibody Technology.

Q Are you able to read the signature on the bottom of this document?

A In fact I can't, no.

Q And no guess as to who it might be?

A Well, it says from Diana Dunstan, but I can't actually read it.

Q Okay. Now, this document suggests that at least Diana was suggesting that there be a study made of who invented what. Do you read the document that way?

A I think that's correct, yes.

Q Did you involve yourself personally in any such study in response to this document or for any other reason?

A I think I've explained to you that I had a discussion in July 1991 where, indeed, I did look into it in some detail, and my own view,

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at this point, was it very much depended on the view you took of the invention. Therefore, in a way, it was premature to discuss the attribution of spoils until such point as we saw the patent claims, and then one could go through the claims and say, "Well, when we've taken proper patent advice, what are viewed as the key claims of the patent, and who is associated with those."

Q Did your view prevail, to your understanding? That is to say that the question of attribution was deferred until the patent claims had been drafted?

A The question of attribution of the commercial spoils, which I think this is what it's about, was indeed to be deferred, and I think that's why the form of agreement took place after the final filing.

Q Did the attribution of contribution, relative contribution, in reference to the claims as drafted ever occur?

A I'm sorry, could you say that --

MR VEZEAU: Yes, would you read that back, Thelma?

COURT REPORTER: Certainly.

"Q. Did the attribution of

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contribution, relative contribution, in reference to the claims as drafted ever occur?"

THE WITNESS: Sorry, can you repeat that again?

COURT REPORTER: "Q. Did the attribution of contribution, relative contribution, in reference to the claims as drafted ever occur?"

THE WITNESS: Can I ask which claims? As drafted at this point? Or as drafted later?

BY MR SKILTON:

Q Let's first ask it generally and then I'll go to some specific claims for a reference point.

A Was there relative attribution?

MR VEZEAU: Can you possibly, John, can you simplify that question a little bit? Maybe you can't.

MR SKILTON: Well, I'm not trying to confuse anybody so let me see if I can do it.

BY MR SKILTON:

Q This document, on its face, as we earlier discussed, calls for an investigation to determine the details of the contribution made by each of those concerned?

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A Yes.
Q You indicated, did you not, that any such investigation was deferred at your suggestion until the claims were actually drafted?
A Well, I don't know whether it was deferred on my suggestion, but I made the suggestion and it was deferred. But I think that Sir Aaron Clug and Cesar Milstein were quite capable of coming to their own conclusions, and I think we all felt -- well, in particular, Sir Aaron and Cesar felt -- that we hadn't seen a copy, or they hadn't seen a copy of the priority document, and, although they believed this to be based on work from the laboratory, they had not seen the detail and, until they knew exactly what was in it, it was very difficult for us to come to a conclusion.
Q So there was no investigation done initially in response to this memo, to the best of your knowledge and understanding, is that correct?
MR VEZEAU: I'm going to object to that question as being indefinite and lacking foundation.
THE WITNESS: You say was there an investigation? I mean, clearly, there were

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discussions that went on as part of these negotiations. You've already drawn my attention to a document from David Chiswell to the MRC, and we now see a letter from the MRC to David Chiswell, so at that level there had been an attempt to make attribution.
The MRC is claiming it should be between 50 and 75 per cent, and Chiswell is claiming it should be between 25 and 50 per cent.
At some later point this figure of the relative attribution was decided as only 50 per cent, so you could regard it as a compromise, but actually it was based on the argument that this had been a genuine cooperation between the two parties, MRC and Cambridge Antibody Technology, and really that they had all made essential contributions for the progress of the -- or for the patent filing, and therefore it should be 50/50.
BY MR SKILTON:
Q And that's as between the two entities?
A Correct.
Q Cambridge and MRC?
A Correct.
Q Now, I'm interested in whether

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you're aware of any investigation, in any form, which attempted to determine what, for example, John McCafferty contributed to any claim in this patent?
A You used the word "investigation". Could you explain what you mean by that?
Q Interview with McCafferty, determination as to whether he was the inventor of claim 1 by either a lawyer or by yourself?
A I think I told you earlier that there had been the July 1991 date. There most definitely had been an analysis of each claim of the patent and an attribution of an inventor to each claim, or multiple inventors to specific claims.
Q And I would like to determine exactly what that analysis showed as of that time, July of 1991. How do I find out?
A I don't think you can because I think this was something which involved David Chiswell and myself discussing the matter in conjunction with our attorney.
Q I'm going to place in front of you a document that was earlier marked as exhibit 9 in the CAT 30(b)(6) deposition which occurred on --

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this will be American -- February 4 of this year.
A I'm sorry, what? What is this?
Q This a copy of the patent.
A What's it to do with?
Q The 108 patent.
A What are you talking about, February 4th?
MR VEZEAU: If you look at the exhibit stamp in the bottom it's written in more American than European form, with the month, day year in that sequence.
THE WITNESS: I understand.
MR SKILTON: I was just doing it for the record. That wasn't a question.
THE WITNESS: Oh I see. So this is nothing to do with the date of the patent?
MR SKILTON: Correct.
THE WITNESS: It's just the time at which this formally enters the proceedings?
MR SKILTON: Yes, so I didn't have to identify it again. That's all.
THE WITNESS: I see, okay.
BY MR SKILTON:
Q Do you recognise this document?
A Yes, I do.

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Q I'm going to take you back, if you will, to the claims of this patent, which you'll know, from reading other patents, is toward the end of the document.
A Mmmm.
Q Dr Winter, I'm now looking at pages 14360, CM014360, through CM014362, columns 121, 122, 123, 124, 125 and 126. Do you recognise this to state the claims in the 108 patent, correct?
A In a general sense, yes.
Q Now, at some point in time, as I understand it, in the application process, you and Dr Chiswell and -- was it Sean Walton? -- sat down with a document and attributed the claims as then written separately to inventors?
MR VEZEAU: Objection. Lack of foundation.
BY MR SKILTON:
Q Do I understand that correctly?
A No, you don't.
Q What is the fact as to what you and Dr Chiswell did with Sean Walton?
MR VEZEAU: Objection. Lack of foundation.
BY MR SKILTON:

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Q Was it with Sean Walton?
A No, it was not.
Q Sorry. Was it with the other lawyer that you named earlier?
A I mentioned Ian Armitage earlier.
Q What was it that you did with him?
MR VEZEAU: Again, we're in the area of privileged communications. If you can answer this generally without getting into the specifics of the information exchanged, you may do so. If you can't, let me know and you should not.
THE WITNESS: I've referred earlier to the process which we went through. I can repeat that, if you want.
BY MR SKILTON:
Q I don't need you to say something you've already said on the record.
A Fine.
Q Did that process result in the attribution of specific names to specific claims?
A Yes, it did.
Q Did your attorney take notes, to your understanding, of the conclusions that were reached when that process was completed?
A I do not know what the attorney

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did.
We had a list. As we went through the list -- as we went through the claims, we drew a list of the people involved with each claim. Furthermore, we also drew up a list of those people in my laboratory and in Cambridge Antibody Technology, and we compared those lists. So with each claim considering who we felt was involved with the claim. And then, secondly, checking this list of names to make sure we hadn't missed anybody out, or incorrectly attributed somebody.
Q Do you recall -- strike that. Let me go back.
There are, according to the patent, and will you look at the first page with me, please?
A Yes.
Q Twelve named inventors. And you can take your time to count them, if you wish.
MR VEZEAU: I come up with more.
MR SKILTON: Do you? What is your number, please? The document speaks for itself.
MR VEZEAU: Of course it does. Let's put it this way; I think we've established CAT can count better.

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MR SKILTON: Thirteen.
THE WITNESS: Can you tell me --
MR SKILTON: My colleague tells me the number is thirteen.
THE WITNESS: I counted twelve, actually.
MR SKILTON: You'll have to strike your last comment, Mr Vezeau.
THE WITNESS: I make it twelve.
MR VEZEAU: It isn't twelve, John. It's a dozen.
BY MR SKILTON:
Q Is this the list that you and Dr Chiswell, together with your attorney, came up with at the session that you have now described on this record?
A Probably. I can't be sure.
Q Does -- strike that.
Do all twelve inventors share equal credit for all of the claims of this patent?
MR VEZEAU: I am going to object to that question because I don't know what it means. Indefinite.
THE WITNESS: Equal credit for all of the claims? So you're implying that all twelve

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were involved to an equal extent on each claim?
MR SKILTON: Yes.
THE WITNESS: Is that what you're saying?
MR SKILTON: Yes, that's my question.
THE WITNESS: I don't believe that -- well, it is definitely the case that there was not equal involvement on each claim of this patent. Obviously some people made more of a contribution than others on specific claims.
BY MR SKILTON:
Q Well, I want to get the process down. I'm not trying to jam conclusions down your throat, so let's work at it indirectly through direct questions.
With respect, for example, to claim 1, what was the process in reference to that list -- that list of both CAT and MRC lab persons -- that you were working on to come to the conclusion as to inventorship with respect to that claim? What was the process?
A Sorry, I'm trying to recollect what the process was on claim 1.
MR VEZEAU: May I hear the question

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again, please?
COURT REPORTER: "Q. Well, I want to get the process down. I'm not trying to jam conclusions down your throat, so let's work at it indirectly through direct questions.
"With respect, for example, to claim 1, what was the process in reference to that list -- that list of both CAT and MRC lab persons -- that you were working on to come to the conclusion as to inventorship with respect to that claim? What was the process?"
MR VEZEAU: I'm going to object to that question as lacking a proper foundation with respect to the direction to claim 1 of the patent as issued.
MR SKILTON: Right. Why don't we take a 5-minute break? I understand the objection. I'll see whether I can push another document here.
MR VEZEAU: Sure.
VIDEOGRAPHER: We're going off the record. The time is 1448(sic).
(A short recess at 3.48 pm)
(Resumed at 4.04 pm)
VIDEOGRAPHER: We're back on the

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record. The time is 1504(sic).
(Exhibit 14 marked for identification)
BY MR SKILTON:
Q Dr Winter, I'm going to place in front of you a document which has now been marked as exhibit 14, which I will represent to you, and we can confirm if necessary, is the first priority document, at least most of which here was filed on July 10, 1990, and by that I mean the pages that commence on CM000351. The whole document bears the Bates stamp numbers CM000348 through CM000383.
I'm going to take you then to the page beginning on 351, if you would, with me. Do you recognise this document?
A I don't specifically recognise it.
Q Is it a document, as you look at it, that you had any role in drafting or vetting?
MR VEZEAU: What was -- vetting?
MR SKILTON: Vetting.
MR VEZEAU: I'm not quite sure what that means in this context.
MR SKILTON: I'll rephrase the question. I'll ask it one at a time.
BY MR SKILTON:
Q Did you have any role in drafting

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this document, that which begins on CM000351?
A I'll have to read it to be sure about that. I think it's quite likely that I would have had some role in some bits of it, but I don't believe that -- I simply can't remember whether I was involved in the final approval of this document.
Q Do you have a belief as to who it was, in fact, that drafted this document?
A Well, I can surmise it was David Chiswell, because John McCafferty was working experimentally and David Chiswell was only really the person with the time to spend making drafts at that point.
Q And realising that you -- strike that.
When was the last time you reviewed this document, if you recall?
A I don't recall.
Q And certainly you haven't reviewed it in the last thirty days?
A No, I have not reviewed it in the last thirty days.
Q So, in order for you to be more specific than the answer that you gave, you would

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need to read the document carefully and think about it for a while, is that a fair statement?
A I would certainly need to do that before I made much more comment on it, because we're going back a long time. But whether or not, if I did that, I would be able to tell you much more about it, I don't know.
MR SKILTON: I'm going to place in front of you, then, exhibit 15, which I will represent to you is the international application under the PCT. For the record, I'll identify it as including pages CM000130 through CM000347.
(Exhibit 15 marked for identification)
BY MR SKILTON:
Q Have you reviewed this document in the last thirty days, Dr Winter?
A I have not.
Q We have helpfully marked with a yellow tag the original claims as they appear in this document, which begin on CM000290. Would you turn with me, please, to that page?
A Yes.
Q Do you have an understanding of what it is you are now looking at here, beginning with the word "claims" and 1 thereafter?

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A This certainly appears to be similar to the claims that I was involved in drafting. Whether identical, I don't know.
Q All right. And responding to Mr Vezeau's objection, do you believe this to be, as you sit here today, the claims that were in front of you at the time that you went through the process of trying to attribute the inventorships specifically on a claim-by-claim basis?
A I simply don't know whether they are or not. I would surmise they are, but I don't know for sure.
Q On the assumption that they are, and now referring you to claim 1, would you describe the process that you and Dr Chiswell and counsel went through in ascertaining who it was that was the inventor of claim 1, or inventors, plural?
MR VEZEAU: Again, do you understand this question calls for a process and not the specific exchange of information that you may have had at that time?
THE WITNESS: Yes. I'm going to have to read this claim first.
BY MR SKILTON:
Q Please.

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A (Witness reviewed the document) It's, kind of, rather difficult to describe the process without describing the detail.
MR VEZEAU: If you cannot do so then you should not do so, because I believe that's privileged.
THE WITNESS: I see.
BY MR SKILTON:
Q I actually disagree with that, but are you able to state anything as to what the process was of ascertainment of inventorship with respect to claim 1 in this PCT?
A I think the only thing I can say is that each claim was looked at individually, and we discussed the various people involved in the demonstration of that claim and the contributions that they had made, but, after this time, it's very difficult to remember the exact process.
I don't even know if there was a single process, or if there were multiple processes. I mean, the ideas went backwards and forwards as we sought advice from the attorneys as to what was relevant and what wasn't relevant.
Q Were you working with any document in this process?

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A I'm not actually sure we were working with a document. I think that we had the claims in front of us on a computer.
Q Was there anyone who recorded the results of this process of attribution on a claim-by-claim basis?
A I imagine there must have been but I don't remember anyone specifically doing it.
Q Can you tell me whether, with respect to claim 1, Hennie Hoogenboom was determined to have been an inventor?
MR VEZEAU: If answering that question requires you to divulge information that you provided to your attorney or that Dr Chiswell provided to his attorney, or an opinion the attorney gave to you, you should not do so because I think that's privileged.
THE WITNESS: If that's correct that rules out the question, because the whole process involved a discussion between myself and an attorney, and Dr Chiswell and an attorney, and the three of us together at least in that meeting.
MR SKILTON: Would you read the last answer back?
COURT REPORTER: Certainly.

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MR SKILTON: With the question.
COURT REPORTER: "Q. Can you tell me whether, with respect to claim 1, Hennie Hoogenboom was determined to have been an inventor?"
Mr Vezeau gave some advice to the witness. The witness said:
"A. If that's correct that rules out the question, because the whole process involved a discussion between myself and an attorney, and Dr Chiswell and an attorney, and the three of us together at least in that meeting."
BY MR SKILTON:
Q What contribution, in your opinion, did Hennie Hoogenboom make to the invention described in claim 1 of this application?
MR VEZEAU: Do you recall my advice to you?
THE WITNESS: Yes, I recall your advice, and it appears to be the same question or am I mistaken?
BY MR SKILTON:
Q I'm not asking here for any involvement of the lawyer. I'm asking here for your opinion as to who at that time, the one that

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you had contemporaneously at that time, as to who was the inventor and, in specific, was Hennie Hoogenboom an inventor of claim 1?
MR VEZEAU: If, in answering that question, you're required to divulge information you provided to your counsel for the purpose of receiving an opinion from counsel as to inventorship, then you should not do so because that communication is privileged.
BY MR SKILTON:
Q Can you answer the question?
A It sounds like I can't answer the question.
Q So you follow that instruction?
A What else am I supposed to do?
Q I just want confirmation on the record. The answer is "yes"?
A Yes.
Q Can you tell us what work Dr Hoogenboom did with reference to claim 1?
MR VEZEAU: I would give you -- to the extent in answering that question entails your revealing information you provided to your counsel for the purpose of receiving an opinion, I believe that's privileged. And I would so advise you, if

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that is the case, you should not respond to that question.
THE WITNESS: Sorry, could you repeat the question?
COURT REPORTER: Certainly.
"Q. Can you tell us what work Dr Hoogenboom did with reference to claim 1?"
THE WITNESS: If you want to know what work Dr Hoogenboom did, I can tell you about the work that Dr Hoogenboom did. But you're asking me to place that in the context of claim 1 and that, I understand, I shouldn't answer.
BY MR SKILTON:
Q Let's take it in the terms you've just given. I want to go through each of these inventors, please, and we'll start with John McCafferty. What work did John McCafferty do?
A If you want to go through each of the inventors, I'm going to have to take some time off and read this thing because, simply, I may forget as to exactly what each person did, and I think that wouldn't be right.
I think you're already aware of the work that McCafferty...
I mean, a simple answer to your

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question is, if you look at the papers that are published, the people on those papers were involved in doing the work somehow.
MR SKILTON: Dr Winter, I can assure you that I am not here to either waste your time or to make this unduly complicated, and I will, in this context, and we're getting out the interrogatory, state that we made an effort here, through proper discovery some five months' ago, to get a narrative answer to these questions by interrogatory.
(To Mr Vezeau) We have not received an answer and I ask counsel whether it is your intent to supply us with an answer to that question 6?
MR VEZEAU: I disagree with you. I believe you have received an answer. You may not like the answer you received, but you did.
In addition, you have testimony in the record from numerous witnesses of the contributions of each inventor. So the legal conclusions you may wish to draw, to me, is a matter of law. I don't think you're entitled to get into privileged communications with counsel. For example, I do recall Dr Jackson, and I believe Dr McCafferty, going into some detail on these

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points. Also Dr Griffiths, I believe, and Dr Johnson.
(Exhibit 16 marked for identification)
MR SKILTON: You need not read that, Dr McCafferty(sic), I'm finishing my record on this point, but I would ask that --
THE WITNESS: I'm Dr Winter.
MR SKILTON: I'm sorry. I apologise to you. My mind is running too fast.
We have marked as exhibit 16 Cambridge Antibody Technology Limited's response to MorphoSys' first set of interrogatories. Earlier I made reference to 6, which I will read into the record, with the answer. The question asks:
"Identify all of the inventors of the 108 patent and for each inventor identify what contributions they made to the invention(s) claimed in the 108 patent."
The response is:
"John McCafferty, Anthony Richard Pope, Kevin Stuart Johnson, Henricus Renerus Jacobus Mattheus Hoogenboom, Andrew David Griffiths, misspelled, Ronald Henry Jackson, Kaspar Philipp Holliger, James David Marks, Timothy Piers Clackson, David John Chiswell, Gregory Paul Winter,

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and Timothy Peter Bonnert, who each contributed to the concept of one or more claims of the 108 patent."
Counsel, is that the answer of Cambridge Antibody Technology?
MR VEZEAU: Coupled with the subsequent testimony you have received, yes. And if you'd like us to supplement a response to refer to that, I'd be happy to do so.
MR SKILTON: Well, I'm not going to tell you how to supplement any response, but I can tell you that I believe this interrogatory is required to have an authoritative answer under the Federal Rules, not to put us in a position here of attempting here, through witness testimony, to reconstruct that which is not reconstructible. And so my statement on the record is that this answer is inadequate as a matter of law.
MR VEZEAU: And, John, if you look at your second to the last set of responses to our interrogatories, they are absolutely inadequate as a matter of law.
I believe this answer is adequate. However, as I said, we will make a note and revisit this and see if we can respond, and call your

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attention to the testimony in the record.
Legal conclusions you want to draw from that, you may argue until the cows come home as far as I'm concerned, but we will draw your attention to the factual testimony of the various witnesses, including the 30(b)(6) witness of CAT.
MR SKILTON: Well, what we asked for, just so it's clear, is CAT's attribution on a claim-by-claim basis of inventorship. Not the attribution of individuals who were not involved in the process, but CAT's. And that is the request that we make, and that is the answer which we wish to receive.
BY MR SKILTON:
Q Dr Winter, would you be kind enough to state your present affiliation, in terms of business affiliations?
A I'm a director of Diversys. I'm director of PepTech.
Q A director of?
A PepTech.
Q That PepTech is the same entity that was early involved in the funding of Cambridge Antibody Technology?
A Correct.

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Q Do you have any current affiliation with Cambridge Antibody Technology?
A Do you mean am I a director? Or do you mean am I an employee? I'm neither of those.
Q Are you a consultant?
A I'm not sure.
Q Have you performed consulting services in the last six months for Cambridge Antibody Technology?
A I don't believe I've performed consulting services of the type that you -- or what I would normally call consultation.
Cambridge Antibody has agreed to remunerate me for the time that I spend involved in this case, and so, therefore, I will receive some kind of a remuneration from them. Whether that, in your eyes, makes me a consultant, I'm not sure, but that's the situation.
Q Can you tell me what the rate of remuneration is?
A I don't recollect it.
Q Have you billed Cambridge for that kind of consulting work to date?
A I've actually -- I billed them in respect of the last repertoire.

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Q By that you mean the last case?
A The last case. I don't think I've billed them in respect of this case.
Q Do you remember how much that bill was?
A No.
MR SKILTON: Counsel, I would ask that a copy of that bill be produced.
BY MR SKILTON:
Q Have you received any compensation -- strike that.
Are you vested in any royalty rights as they may relate to the 108 patent?
A Royal rights. Can you define what you mean by "right"?
Q First let me backtrack and ask. Have you received any payments, royalty or otherwise, in reference to the 108 patent, other than those which you may receive as a consultant?
A Perhaps I can give you the formally correct answer, which is that the Medical Research Council operates an award to inventor scheme, and I'm an inventor on a number of patents from the Medical Research Council. And when the Medical Research Council receives revenues for an

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individual patent or a bundle of patents, as in the case of the patents that were licensed to Cambridge Antibody Technology, the revenues are distributed mainly to the Medical Research Council itself. It keeps the lion's share, but some fraction goes to inventors.
So it's not clear to me whether I have a right. I think it's based on a process of recommendation and a process of assumption about the respective strengths or importances of different patents to a portfolio of patents.
But the essence of it is that I do believe that from, within the money the Medical Research Council makes itself, which recently has been its share sales of equity in Cambridge Antibody Technology, that I will receive some fraction of the inventor's cake.
Q To date, do you believe that you have obtained some fractions as a result of the 108 patent?
A Yes, I do.
Q Do you have knowledge of how much money you've received as a result of the 108 patent?
A As I said, the process involves

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a recommendation and, in the end, I get a number. I do not know exactly how the Medical Research Council does its calculation, but I have reason to believe that they follow a process which attributes inventors -- it attributes percentage -- start again.
It takes, first of all, the idea, in the case of Cambridge Antibody Technology that there's a bundle of papers that were licensed to them. It then attempts to dissect the relative importance of those papers from among the bundle. And then, with respect to each invention, it looks at all the inventors involved, those from the Medical Research Council, and there's some kind of attribution of the relative roles of those people. The process is obviously imperfect, and it does get reviewed from time to time.
Q Who is it that makes these calculations at the Medical Research Council?
A This is the Medical Research Council head office on the basis of recommendations made by the director of the laboratory.
Q Are these recommendations ones that you can review, if you request to review them?
A I don't know.

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Q Have you ever seen documentation of the process or the result as they relate to your payments concerning the 108 patent?
A I was involved in formulating an equitable reward proposing to the MRC a mechanism by which we might deal with the attribution of rewards from those patents, but that process went through various review stages. I have reason to believe that they followed something resembling my initial recommendations.
Q Were your recommendations in writing?
A There was a draft document in writing, correct.
Q Did it comment specifically on the 108 patent in terms of its relative value in the bundle?
A It would have commented on it, yes.
Q Did it assign a relative value as a matter of percentage of the total revenues to the 108 patent?
A You refer to the 108 patent, but in fact there's obviously a European filing as well, and so I'm talking about the -- by 108 patent, I assume you mean the whole body of work relating

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to phage display and repertoires.
Q Rather than me guessing, since I haven't seen it, why don't you divide it out to the best that you can, as to what are the divisions in this bundle as it relates to the various pieces of intellectual property? And how were the divisions made? And how does it relate to individual inventors?
A I can give you a broad answer.
There are two patents or two patent -- let's say two families of patents. The first patent or family of patents is the repertoire technology patents. And obviously this has European/American counterparts, etcetera, or Japanese counterparts for all I know. That's one of those families.
And the other one is the phage display of antibody fragments, of which display of single chain Fvs is part of it.
Now, there were a series of other patents which also were licensed to Cambridge Antibody Technology, and, at the beginning, we started off with the assumption that the key things that were licensed to Cambridge Antibody Technology were those two families of patents I have mentioned

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to you; the repertoire and the phage patents.
So we took as our first proximation -- let's say it's 50 per cent each. Because, basically, the value of Cambridge Antibody, which is based on -- which reflects -- sorry, start again.
What we tried to look at was there are obviously different ways in which the Medical Research Council might receive revenue from these families of patents. The first one related to the value of those patents to Cambridge Antibody Technology in respect of equity.
The second aspect is in respect of royalty income from the licensing of those patents.
The particular issue that we addressed at the beginning related more to the value of the whole bundle to the share price of Cambridge Antibody Technology, and that, in that context, it was felt that most of the value, at that time that we did this review, related to the repertoire and to the phage display.
There were other patents which we thought might be important, but at that stage a much more minor value was put on them. I can't

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actually remember now exactly what percentage we'd attributed to the phage display of antibodies, so that includes the display of single chain Fv, its various jurisdictions, but my guess is it was probably, in the end, somewhere between 30 and 40 per cent.
Q And someone, I take it, worked with your recommendations to have a final determination as to how these divisions would be made, is that correct?
A Yes. Sir Aaron Clug looked at the proposals I'd made. And we had some discussion about whether they were right, and I explained to him the various problems that I was struggling with in the recommendation. I simply can't remember how much Sir Aaron had in the general process, but I would say that I provided the initial view and that was refined in discussion and debate with him.
Q Is this a process that is still in place today for purposes of the distribution of revenues by MRC to its inventors?
A The MRC has a system of awards to inventors which is normally not grappling with a problem as complicated as this.
Normally, I think when they had

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their scheme, the idea was that the -- or when the scheme was set up, that there would be revenue from a particular patent, royalties from the Medical Research Council -- sorry, to Medical Research Council, and that they could then distribute the revenues arising from that patent or, for that matter, upfront payments. And, indeed, that was...
I also receive income from a patent on humanising antibodies. And that process attempted to define for each patent the licensing revenue that was being obtained directly by the Medical Research Council.
So, in that case, each patent is relatively isolated. If you look at each patent on its merits, you say, "This amount of revenue is coming in for that particular patent."
With Cambridge Antibody, we had the problem that the patents were licensed as a block, and, indeed, were also...
So there were licensed blocks whenever we had to do some attribution of the values of those patents at the time they were licensed to Cambridge Antibody. And I think the view that was taken was that, in respect of equity sales of MRC -- sorry, the sale of MRC shares in

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Cambridge Antibody, that the value or the percentage should be based on the perception of those patents at the time that they passed to Cambridge Antibody Technology. In other words, Cambridge Antibody's perceived value.

And I think, at that stage, Cambridge Antibody saw really the phage and the repertoire patent as being the major components, and the others were desirable.

I think the anti-self patent at that stage seemed to be important, but not as important as the other two.

At a later point we were obviously going to have to confront the issue of -- you have to remember that many of these were patent applications; we didn't know in exactly what form they'd be granted. But, nevertheless, a value had been attributed of that block of intellectual property at the time the agreement with the Medical Research Council was signed. And I think the key date was taken to be before floatation because, at that point, the MRC received -- essentially passed over a whole block of intellectual property and, in return, had its shareholding taken to about 10 per cent of the value of Cambridge Antibody

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Technology. Previously it had been about 4 per cent.

So we have not yet dealt with the... So far as I'm aware, we haven't got royalty income at the moment. I say "we"; Cambridge Antibody Technology doesn't have royalty income. And the MRC, therefore, doesn't have a corresponding fraction of that. We don't yet know which of the patents are seen by the market to be the most important, and that's a question that will have to be revisited. The MRC is going to have to work out how it wishes to deal with royalty income when, maybe, some patents now, with the efflux of time, appear to be much more important.

Q To your understanding, there exists at the MRC some kind of a formula that permits distribution in the event that royalty income is received. To your understanding, there is some kind of a formula?

A No, I don't have that understanding. I have the understanding that -- or certainly what I had suggested was that the matter would have to be revisited with royalty income arose, and to see which patents were actually involved, so far as the MRC could determine, in the generation of that

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royalty income.

Q Did your initial work also attempt to ascribe relative contribution of inventors on given patents?

A Yes, it did.

Q Did you do so expressly as it relates to the McCafferty phage display patent?

A The phage display patents, you mean the family of those?

Q Yes.

A We didn't break it down into whether it was single chain Fv or fab or VH domain or whatever.

Yes, we did, but I should comment that the MRC was only concerned with those people who were MRC employees.

As the result of an agreement between Cambridge Antibody Technology and the MRC, essentially the MRC did not compensate John McCafferty. So John McCafferty, in fact, was compensated within the company, Cambridge Antibody Technology, for the contribution he'd made to the early success of the company, by issue of further equity.

He had felt that his initial issue

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of equity had been rather low and that, as a result of his invention, he should be compensated further. At some level we agreed with that and, in fact, we compensated him generously.

So the MRC felt that, of its awards to inventors scheme, it was not quite correct to say McCafferty, as he was a company employee and, in any case, the company had taken care of him.

MR SKILTON: Counsel, we will be requesting the documents that have been described by this witness.

May we take five minutes?

VIDEOGRAPHER: We're going off the record. The time is 1543(sic).

(A short recess at 4.43 pm)

(Resumed at 4.55 pm)

VIDEOGRAPHER: We are back on the record. The time is 1555(sic).

BY MR SKILTON:

Q Dr Winter, earlier I asked you some questions about George Smith, and I think you indicated you had never met him?

A As far as I know I've never met him.

Q Is it also true that you never heard him present in any context?

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A I'm quite confident I've never heard him present.
Q Do you know whether anyone at CAT has ever heard Dr Smith present on the issue of phage display?
A To the best of my recollection, I don't think anyone had heard Smith talk about phage display. You mean phage display of anything?
Q Anything. Phage display? Short peptides?
A I simply -- I don't remember anyone commenting to me that they'd heard him.
Q And that would be true concerning MRC as well?
A You mean MRC inventors?
Q Yes. Anyone at MRC, that you're aware of?
A Oh, that I'm aware of?
Q Yes.
A Look, I haven't a clue. I'd have to think about it. Anyone at the MRC is a huge volume of people.
Q Right. I'm asking in terms of your knowledge, realising that if it didn't come to you you wouldn't know about it.

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A Well, not necessarily. I mean, you know, it could be that someone from the MRC may have gone to see it and I might have heard about it but I don't remember it now. But certainly I have no specific recollection, at this moment, without any further prompting, of anyone from the MRC going to hear Smith. But they must have done because, you know, Smith is around there, and there's people have heard Smith or met Smith, but I don't specifically remember specifics.
Q Just to finish this off. With respect to Chiswell, do you know whether or not he ever heard Smith talk?
A I don't know.
Q And with respect to McCafferty, the same question?
A The same answer.
Q Let's go to Dr McCafferty's work in your lab before CAT had its open lab.
You mentioned in your earlier testimony there was an oral agreement concerning intellectual property. Did I hear you correctly, with McCafferty?
MR VEZEAU: With McCafferty? Or with CAT?

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MR SKILTON: As it relates to the work of McCafferty.
MR VEZEAU: But agreement with whom?
MR SKILTON: Let's break it down.
BY MR SKILTON:
Q Was there an agreement with McCafferty directly that you're aware of?
A There was an agreement, the visitors' form. The MRC visitors' form, I believe, specifies that it's up to the Medical Research -- that, to the extent that McCafferty has any rights, that the MRC will compensate him according to the awards to inventors scheme. I believe that's what it states.
Q Does that require him to list -- I'm sorry.
A And it will deal with his employer, to the extent that the employer has any rights. That is what I understand. I don't have the detailed wording here, but that's the intention.
Q That means, does it not, that McCafferty, if he's to invent anything while in the MRC lab, is to assign the rights to that to the MRC?

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MR VEZEAU: Objection. First of all, that calls for a legal conclusion from this witness. Secondly, that lacks foundation in fact or in law, particularly UK law.
MR SKILTON: (To the witness) Do you know?
THE WITNESS: Can you repeat the question again?
COURT REPORTER: "Q. That means, does it not, that McCafferty, if he's to invent anything while in the MRC lab, is to assign the rights to that to the MRC?"
THE WITNESS: I'd have to see the form to remind myself exactly what it's saying, but my understanding was what I've given you earlier.
BY MR SKILTON:
Q The form controls that question, to the best of your knowledge?
MR VEZEAU: Objection.
THE WITNESS: The form controls --
MR VEZEAU: Excuse me just a second. Lack of foundation for the same reasons I stated previously.
THE WITNESS: I don't understand exactly what's going on here between the lawyers,

but my understanding is that it depends really who else is involved in it. Obviously there were discussions going on with David Chiswell at that time, so that I think that -- I don't think we had any understanding that, if McCafferty discovered something in the lab, that it all passed to the MRC. I think that we were clearly of the view that (1) his employer had rights and (2) that David Chiswell was a part of that process. So I don't think that we could -- and was intimately involved in the experiments that McCafferty was doing.

BY MR SKILTON:

Q Was there any oral agreement other than the form 7 issue that you've earlier testified to?

MR VEZEAU: First of all, John, there's no form 7, and if you have a form that's not --

MR SKILTON: I don't have a form. It's not here. It was referred to in a document that you produced.

MR VEZEAU: It's a form Y.

MR SKILTON: Thank you.

MR VEZEAU: And a form Y would not be in the oral agreement. Dr Winter has discussed

an oral agreement between MRC and CAT.

BY MR SKILTON:

Q What was that oral agreement, Dr Winter?

MR VEZEAU: Objection. Asked and answered.

BY MR SKILTON:

Q What was that oral agreement?

A Sorry, the oral agreement between MRC and Cambridge Antibody? Is that what mean? Or are you --

Q Yes, what was that agreement?

A Right. My recollection of that is that the agreement related to the work emerging from John McCafferty. There was an understanding that, to the extent that both parties were involved in it, that both would get any commercial benefits from it, and that the thing was seen as a joint collaboration between the two.

Q To whom would McCafferty assign his rights?

MR VEZEAU: Objection. Again that's calling for a legal conclusion from this witness. He's not a lawyer.

BY MR SKILTON:

Q Can you answer that question?

A Well, again, I'm not quite sure exactly what the question means. To whom McCafferty would assign his rights?

Q Under the oral agreement.

A Under the oral agreement?

Q Yes.

MR VEZEAU: Objection. Lack of foundation.

THE WITNESS: I simply don't know what the detailed technicality would be. That's law and I guess patent law as well. But I think I've indicated the agreement that we had, it related to the body of any IP being produced, would be if the respective parties, that is Chiswell and -- sorry, start again.

If McCafferty came up with something in the lab. In other words, let's suppose Chiswell hadn't got any involvement in at all. If McCafferty had come up with something in the lab, and let's suppose Chiswell hadn't made any inventive contribution whatsoever, then that would be jointly owned by MRC and Cambridge Antibody Technology. So that was the understanding. Things arising from McCafferty's work in the laboratory

would be jointly owned.

What emerged was that Chiswell also made a contribution to the particular phage display project, so that consolidated that understanding.

BY MR SKILTON:

Q Was the agreement that you are describing here one which required assignment, then, of anything developed to both entities?

A I simply don't remember the technicalities of it. As far as we were concerned, we were discussing the outcome of anything that McCafferty would do in the laboratory and the fact that that would be shared by the two parties or jointly owned by the two parties.

Q Do you have knowledge as to whether or not this question of assignment of rights or ownership was one that was investigated by Sean Walton?

A I have no idea.

Q Or Ian Armitage?

A I don't know.

Q Is it correct to say that, as you sit here today, you simply do not have knowledge of what legal obligation John McCafferty would have to assign any invention made in the MRC lab during

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this period where he was covered by an oral agreement to either Cambridge or the MRC?
A That question is quite a complicated question, but John McCafferty... There was a written agreement relating to John McCafferty's rights, such as he personally may have, which I have mentioned already on the form Y.
Q Form Y?
A Which he agreed, as far as I recollect, from that form that -- it's a fairly standard form -- to assign such rights as he may have personally to the MRC for work that was being undertaken in that laboratory.
MR SKILTON: Counsel, if it's possible, I would like to have that form Y available to me when I question Dr Chiswell tomorrow.
MR VEZEAU: It probably is not going to be possible.
MR SKILTON: Because?
MR VEZEAU: Because it won't be possible.
MR SKILTON: We're in England. Dr Chiswell comes tomorrow.
BY MR SKILTON:

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Q Where do you understand this form Y to be housed, Dr Winter?
A I don't know. The form Y we certainly got signed because Sir Aaron Clug was very keen that that was done before McCafferty came into the laboratory.
Q Do you have knowledge as to whether or not the MRC inventors, that are listed on the 108 patent, were required to assign to the MRC?
A Sorry, were required or did?
Q Well, first let's talk about the question as asked. Were required?
MR VEZEAU: I'm going to object to that because I think it calls on Dr Winter, of course an eminent scientist, to draw legal conclusions, and I don't think he's qualified to do that, in answering this question.
BY MR SKILTON:
Q Do you know the answer to that question?
MR VEZEAU: Do you want to hear the question again?
THE WITNESS: Yes, please. Thank you.
MR VEZEAU: Thelma, you're up.

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COURT REPORTER: "Q. Do you have knowledge as to whether or not the MRC inventors, that are listed on the 108 patent, were required to assign to the MRC?"
THE WITNESS: I believe all of them signed form Y, but what the conclusion of form Y is, I don't know.
I had assumed that this would mean that they should assign their inventions to the Medical Research Council, but I don't know whether -- to what extent that was a requirement or not.
MR SKILTON: All right.
Counsel, I'll conclude, then, the examination with a request that, before Dr Chiswell in particular, so I may have a witness to question on these documents, that the form Ys for all the MRC inventors be produced.
MR VEZEAU: Since Dr Chiswell is going tomorrow, that will not be possible. I can tell you that.
And also, with respect to Dr McCafferty, we with check with CAT. CAT does not have a copy of form Y. So that's why I'm telling you we cannot produce that tomorrow.
BY MR SKILTON:

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Q Are the form Ys MRC documents?
A The form Y is an MRC document.
MR SKILTON: I would request that MRC, which is I believe a party to this litigation --
MR VEZEAU: The MRC is not, at your insistence. We attempted to do so and we brought that to the court's attention, and you were opposed to that, if you will recall, John --
MR SKILTON: I think we are mixing our cases right now, but let's not argue about it.
MR VEZEAU: I don't believe so.
MR SKILTON: I think we are. I think you are but...
MR VEZEAU: I think you forget the motion we filed on that point, which you opposed.
MR SKILTON: But, in any event, I ask that those forms be requested of MRC and produced while we are in England.
MR VEZEAU: I have already told you that will not be possible. My statement stands. Are you through?
MR SKILTON: Yes.
MR VEZEAU: Dr Winter, we thank you. This has been a long day, and we appreciate

you taking time out of your schedule to appear here.

For that reason, I am not going to ask you questions at this time, but simply ask that, when Thelma produces her flawless copy of the transcript of your deposition, that you review it, make any corrections that you believe should be made, and sign it. But you need not appear in front of a notary; we've agreed to waive signature in front of a notary.

THE WITNESS: Thank you very much.

MR SKILTON: Thank you, Dr Winter.

THE WITNESS: Thank you.

MR VEZEAU: And also on the record I want to designate the transcript of this testimony, and the videotapes, as highly confidential under our protective order.

VIDEOGRAPHER: This concludes the deposition of Dr Greg Winter. The number of tapes used was three. We're going off the record. The time is 1612(sic).

(Deposition concluded at 5.12 pm)

ERRATA

CORRECTION PAGE

Signature Date

I, Dr Gregory Paul Winter, am the deponent in the foregoing deposition. I have read the foregoing deposition and, having made such changes and corrections as I desired, I certify that the transcript is a true and accurate record of my responses to the questions put to me on Monday, 29th April 2002.

Signed

GREGORY PAUL WINTER, PhD

CERTIFICATE OF COURT REPORTER

I, Thelma Harries, Accredited Court Reporter, Member of the British Institute of Verbatim Reporters, do hereby certify that I took the stenotype notes of the foregoing deposition, and that the transcript thereof is a true and accurate record transcribed to the best of my skill and ability.

I further certify that I am neither counsel for, related to, nor employed by any of the parties to the action in which this deposition was taken, and that I am not a relative or employee of any attorney or counsel employed by the parties hereto, nor financially or otherwise interested in the outcome of the action.

THELMA HARRIES, MBIVR, ACR

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